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I. *On the Phenomena exhibited by Dusty Air in the neighbourhood of strongly Illuminated Bodies.* *By* OLIVER J. LODGE *and* J. W. CLARK *.

PART I.

[Plate I.]

IN the course of a lecture on Dust and Disease given at the Royal Institution in 1870, Dr. Tyndall calls attention to, and fully illustrates by experiment, a dark or dust-free region which he had observed in the convection-currents rising from hot solids placed in the path of a powerful beam of light (see Proc. Roy. Inst. vol. vi. p. 1, also 'The Floating Matter of the Air'). He describes this dark stream as of surprising sharpness and definiteness, especially when it is seen above an ignited platinum wire, the line of sight being parallel to the wire but at right angles to the beam. Dr. Tyndall also gave two explanations of the phenomena—one of which he considered to be applicable when the solid is at a red or white heat; the other applicable when the body is at some more moderate temperature, such as that of boiling or even warm water. The first explanation is that the dust is absolutely burnt and consumed by the heat; the second is that the hot

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body warms the air in contact with it, which air therefore rises, dragging the dust after it but getting a slight start in advance of the dust, so that a thin stratum of the advance air from either side of the body is free from dust, and the mingling of the two strata constitutes the dark plane. This goes on continually as long as the convection-currents are produced by the body; and so the dark plane is permanent while the body is hot.

Prof. Frankland, in another paper on Dust and Disease, gives a still simpler account of the matter, and considers (Proc. Roy. Soc. vol. xxv. p. 542) that the observation proves that "a very large proportion of the suspended particles in the London atmosphere consists of water and other volatile liquid or solid matter." In other words, Prof. Frankland considers that the dust is simply dried up by the heat.

These three explanations seem to have been sufficiently plausible to satisfy those who may have examined or exhibited the phenomena discovered by Dr. Tyndall, until in 1881 Lord Rayleigh repeated and extended the original observation, "not feeling satisfied with the explanation of the dark plane given by the discoverer" (Roy. Soc. Dec. 21, 1882; 'Nature,' vol. xxviii. p. 139). He used a glass box to prevent draughts, and his hot body was usually a small copper spade which could be warmed from the outside of the box with a spirit-lamp. He called attention to the fact that the stream-lines round the obstacle follow the electrical law of flow, because the warm obstacle is itself the origin of the motion. He showed that smoke was not evaporated by being blown through a hot glass tube into sunshine, and he conclusively disproved any evaporation hypothesis by reversing the whole phenomenon: cooling the rod instead of heating it, and causing the dark plane to stream downwards.

Lord Rayleigh further suggested an hypothesis of his own to account for the dark plane in a simple mechanical manner, viz. that the curvature of the stream-lines near the surface of the obstacle was such as to cause the heavier dust-particles to be thrown outwards away from the body, and thus to leave a thin layer of air free from dust. To test this hypothesis he made a special centrifugal experiment with a whirling table, the direct result of which was negative; but it led to the

observation of an apparent purification of air by contact with a solid, which if followed up might have led a good deal further. It was not followed up, however, though the remark is made that "it would seem as if this kind of contact was sufficient to purify the air without the aid of centrifugal force;" and the paper concludes with another test of the centrifugal force hypothesis, concerning which, finally, "no absolute conclusion can be drawn."

In the autumn of 1883 our attention was called to the matter by Lord Rayleigh's article in 'Nature;' and being struck with the apparent total collapse of all explanations hitherto offered, we proceeded to repeat the experiments with some care. The first result of any importance which we obtained was the fact that the dark plane hitherto observed to rise from a warm body, or to sink from a cold one, is only a continuation of a dark or dust-free *coat* of uniform thickness and sharp outline which completely invests the body; and we were led to the conclusion that this coat is the most important part of the phenomenon, because the up or down streaming-planes seem only to be that portion of the coat which is being continuously wiped off by convection-currents, the coat on the body being as continually renewed by some action not by us then understood. This fact, together with a few other results having reference mainly to the effect of electrifying the solid body, was communicated in a letter to 'Nature' (26th July, 1883, vol. xxviii. p. 297).

Since then we have continued the observations. We shall first describe generally the methods of experiment and the phenomena observed.

General Methods of Experiment.—In all cases the electric light has been employed to illuminate the bodies under examination in dusty media. For the examination in air and gases two principal forms of apparatus have been employed. Plate I. fig. 5 shows the glass box which has been used for ordinary air. The sides and one end are of glass, the other end is of wood and perforated with a hole for a cork; the top is also of wood, but provided with a wide slot, which can be closed by a glass plate, so as to allow the substance under examination to be illuminated from above, or by perforated wooden covers through which tubes, wires, &c. could be introduced when it

was desired to test the electrical or other condition of the body under examination. The box is closed at the bottom by standing it upon a blotting-paper pad on one of Quincke's adjustable glass-plated supports. A fragment of magnesium wire burnt beneath the box served to introduce air laden with magnesic-oxide particles; and by removing the cover from the top of the box tobacco smoke could be readily blown in, or ammonic-chloride smoke by a current of air directed into the box through a heated tube containing ammonic chloride. The body under examination was supported near the glass end of the box either by means of the small adjustable clip shown in fig. 7, or passed through the cork in the opposite wooden end of the chamber. Fig. 6 illustrates this mode of support, the thin line representing a fine platinum wire attached to two stout copper wires through which a current to ignite the wire could be passed. Metal rods thus supported could be directly heated from without by means of a gas-flame, and, if desired, be insulated by passing through glass tubes in the cork.

Ordinarily the convergent beam of the electric light heated the body sufficiently, especially when it had been previously blackened in the smoke of burning camphor. For the examination in dry air and in different gases, either at the atmospheric or at lower pressures, the more complicated apparatus shown in fig. 8 has been employed. The larger end of the horizontal observing vessel is closed with a plane glass plate, through which observations were made. The other end of the vessel was closed with an india-rubber cork carrying two thermometers. The one served to indicate the temperature of the air within the vessel; the cylindrical bulb of the other was covered with camphor smoke and served as the surface upon which the coat formed, and also indicated its temperature approximately. Inert gases were dried by passing through concentrated sulphuric acid and through asbestos mixed with phosphoric anhydride; others, like ammonia, by passing through finely divided calcic oxide and caustic potash. The gases then passed through a wide tube containing ammonic chloride into the experimental chamber. By applying a gentle heat to this tube of ammonic chloride, the gas could readily be charged with its particles. For pressure in excess

of the atmospheric, the form of apparatus shown in fig. 9 has been employed.

For the examination in liquids the box shown in fig. 10 was constructed. It has two plain glass sides. The double brass tube was about 0·5 centim. in diameter, and passed through a cork in the back of the box: this tube was heated either by a current of steam or by simply converging the beam from the lamp upon it. To render the liquid turbid, particles of dried ferric oxide have been found to answer well. With this form of apparatus, as also with those previously described, the light has been passed in through the side of the vessel, the body being examined through the glass in a line with its axis but at right angles to the direction of the beam of light.

The appearances presented by the body were observed either by the eye, through a hand lens, or through one of Quincke's microscopes provided with a micrometer eye-piece, indicated in fig. 8.

General description of Black or Dust-free Coat and Plane.—Before proceeding to a more detailed description of the appearances about to be described under varying conditions, we will briefly state what occurs when a rod of electric-light carbon, in ordinary air holding magnesic oxide in suspension, is illuminated at right angles to its axis at a place near the observed end by the converged beam of the electric light, and is viewed in a line with its axis. A water-cell conveniently intercepts a part of the radiation and renders the action slower. A careful examination at the first instant of turning on the light will usually reveal the dust-particles in close proximity to the surface of the rod, but in the case of a black solid they almost instantly leave it, and after a barely appreciable interval of time the upward and rapid convection-currents commence. There is now a thin ring of perfectly dust-free air and of great definiteness surrounding the carbon rod, a little thicker on the illuminated side, but distinctly traceable all round. On the top of the rod this ring is seen to stream upwards, the up-streaming portion being at first broad, but rapidly narrowing into a black line of very appreciable width, which is often traceable for a long distance through the turbid air of the box. The black ring surrounding the carbon cylinder is the dust-free *coat*, the

black line rising from the top of it is the dust-free *plane*; the junction of the two the *base* of the plane.

On the under surface of the rod the particles are seen rushing up to the edge of the dark coat, not entering it, but curling away to the right and to the left, as if this coat were the real surface of the solid. Fig. 1*a* shows the rod in section surrounded by its coat and the plane on the top; fig. 1 shows also the paths followed by the dust-particles. It will be observed that the dust-particles follow the air-stream lines, moving *with* the air, not *through* it : any considerable motion of particles across stream-lines would soon obliterate the dark plane, whereas it is very persistent and is seen sometimes coiling itself up into a spiral. Another point of much interest is the distribution of the velocities of the dust-particles at different distances from the surface of the solid on a horizontal line perpendicular to its surface. In the case of carbon the velocities are all so great that individual particles are not readily traced, but under favourable circumstances it may be noticed that the particles reach their maximum velocity at an appreciable distance outside the black coat. It may be of interest here to state that on the surface of ice the coat and plane are absent, and the velocity distribution, considered as above, is apparently uniform up to a point so near the surface of the solid as to be indistinguishable from it with any degree of certainty.

We must now proceed to consider these appearances in greater detail, and the conditions under which they undergo modification. The contents of this paper may be conveniently summarized under the following heads:—

1. Experiments made with a view to ascertain the influence of size, shape, and nature of surface of the body examined.

2. Experiments made with a view to ascertain the effect of temperature, electric potential, and other conditions of the body.

3. Experiments made on bodies of different material.

4. Experiments made to observe the effect of pressure on the smoky medium.

5. Experiments made to examine the behaviour of different kinds of smoke—(α) volatile, (β) non-volatile.

6. Experiments on bodies which themselves give off smoke or vapour.

7. Experiments made to examine the conditions under which dust settles on surfaces.

8. Experiments made in media of different natures, *e. g.* different gases and liquids.

1. *Experiments on the Influence of Size, Shape, and Nature of Surface.*

(a) *Size.*—To see whether the size of the body had any marked effect on the width of the dark coat and plane, we have used cylinders of various thickness (*e. g.* iron wires ·15 and ·4 centim. diam., carbon rods ·7 and 1·22 centim.); but without temperature-measurements it is useless to exactly specify the results. We judge, however, that the thickness of the dark coat is independent of the size of the body (other things being the same), but that the rising dark plane is best marked on rods of moderately large diameter; certainly its base is bigger.

(b) *Shape.*—In our early experiments, when examining the appearances of the dark plane, we used copper scraps cut and bent into very various shapes; but we found the dark coat on all of them, and that the up-streaming of the dark plane was a simple matter of stream-lines; so we need only specify the results for a few of the more important shapes. In the great majority of cases we used a simple round rod, and its appearance has been already described. A flat horizontal plate or spade, a centimetre or less in width, shows a coat all over its under surface, very thin coats at its edges where the currents are violent, and a dark plane rising from a broad base, continuous with the dark coat on its upper surface. On a horizontal flat copper plate, 6 centim. wide, the coat on the upper surface is very thick, being little disturbed by currents. Its outline is not perfectly sharp, and it hovers or wavers about, some portions being thicker than the rest, and appearing likely to form upward streamers, though they seldom do. There is a certain amount of up-streaming from the coat, mainly from near the edges. On perforating the plate with a hole of about 0·07 centim. in diameter, a thin line of dust-laden air rises through the black coat on the under surface

of the plate, and emerges into the black coat on the upper surface like a miniature volcano. If it emerge into dusty air the small smoke-column is sometimes seen thinly edged with black from having passed so near a solid.

A vertical flat plate has good coats and a plane. With thin mica or copper, ·003 inch thick, the coats are rather thicker near the top of the plate than near the bottom, and the dust touches the plate near the bottom, as proved by a deposit of dust which forms there. Blackening the surface increases the thickness of the coats.

A hemicylinder of sheet-copper was examined (*a*) with the concave side turned towards the light, (*b*) turned upwards, and (*c*) with the concave surface turned downwards. Its appearance in position *a* is shown in fig. 2; its interior is lined with a good dark coat. The coat was first seen on this piece. In position *b* a coat surrounds the surface, and from the two upper edges rise dark planes. We illuminate this from above by a 45° mirror. On the inner surface the definiteness of the boundary of the coat is not so good. Position *c* gives a dark plane from the top much as a round rod does ; and inside also the dust-free coat is thick and well-marked. Wishing to make the air inside more stagnant, we glazed in its ends with mica, so as to stop even longitudinal currents, and now the gravitational settling of the dust broadened the inside dark coat ; but the coat narrowed to its customary width when convection-currents were caused to circulate within the cavity. The different thickness of coat on curved surfaces is well shown by using the hemicylinder of glass, shown in fig. 4. The coat is evidently thickest where gravitation assists it, and thinnest where it opposes it.

The behaviour of very thin films is curious. Vertical glass films about ·0003 inch thick usually show no dark coat at all, and they either set up no convection-currents, or those which they set up are exceedingly sluggish. Strips of ordinary glass give good coats, the one on the illuminated side appearing first. It seems as if very thin films were incompetent to absorb enough radiation. (It has been incidentally noticed that these glass films, when freshly blown, adhere together when placed in contact. After standing in the air for a few hours this does not take place.)

A lump of rock-salt in ordinary air exhibited dark coats and a plane, but this might be due to moisture on the surface. A good plate of clear rock-salt, in air thoroughly dried with phosphoric anhydride, behaved just like the thin glass films, giving no coats and doubtful convection-currents. A plate of mica 0·002 inch thick absorbs sufficient radiation to exhibit the effects, though when the sheet was horizontal the coat on its upper surface was very badly marked. Mica showed an unexpected result, which is described later on.

Horizontal glass tubes filled with smoke, and immersed in the beam, have been examined by looking along the axis. The air inside is nearly stagnant, but the smoke collects in the axis of the tube, leaving a clear dark space or ring all round. The smoke settles also more slowly when the glass is thus warm than when it is at the temperature of the air.

(c) *Nature of Surface.*—Of three copper rods, one was tightly covered with blotting-paper, one with cotton wool, and the third left uncovered. They were all smoked with camphor-black, and exposed to the beam: the cotton-wool surface showed slightly the broadest plane. But the effect of such variation of surface is not marked, and is probably insignificant. Other experiments made with smooth electric-light carbon, and carbon roughened with sand-paper, led to the same conclusion.

2. *Experiments made on Solids at different Temperatures.*

To determine roughly the heating-power of the beam of light used, and to see at what temperature the dark plane became distinctly visible, a thermometer with its cylindrical bulb blackened with camphor-smoke was inserted horizontally into the box (fig. 5), and used as the solid to be examined. When the smoke was introduced, the thermometer read 21°. A few seconds after the light had been turned on, the dark plane was visible, and the thermometer read 21°·2. At 21°·5 the plane was good, but there was no coat visible to the eye. At 22° the coat appeared, and at 23° it was distinct. The thermometer was still rising rapidly, and soon attained 35°. The water-vessel, which had been used to moderate the violence of the converging electric beam, was then removed, and before long the thermometer was at 100° C., and was still rapidly

rising. The coat and plane were now thick and exceedingly well-developed. In a second experiment, and examining through a hand lens, the coat was visible at 23°·7, and at 26° it was fairly thick. It must be remembered that the whole of the bulb was not warmed by the beam, and these temperature-readings are therefore only rough indications.

Probably common wood-charcoal gives the thickest coat of any substance we have examined ; and although so bad a conductor of heat, the coat is not confined to the part of the solid immediately exposed to the beam. The thermometer was withdrawn from the experimental box, and the brass steam-tube out of the water apparatus (fig. 10) was inserted in its place ; its surface was platinized. When this tube was heated by steam, the coat was rather thicker than when heated only by the beam from the lamp. The thickness of the coat on the steam-heated tube differs little from its thickness on the lamp-black thermometer-bulb at 100°.

The direct effect of temperature is ordinarily inseparable from the secondary effect of the convection-currents, which increase with the difference of temperature between the solid and the air. When the temperature of the solid is high, the coat is thick, although the convection-currents may be very rapid ; for the cause which gives rise to the convection-currents also gives rise to the black coat : and it appears that the increased temperature facilitates the formation of the dark coat more than the increased velocity of the convection-currents thins the coat. Air-currents can be produced by blowing through a capillary tube on to the surface of the rod under observation, and the effect of such a current is to reduce the thickness of the coat ; in fact the coat may thus be rendered so thin as to be imperceptible. The rod may be replaced by a metal tube, closed at one end and pierced laterally by a fine longitudinal slit or small hole in the examined region. An india-rubber tube attached to the other end of the tube allows air to be blown into the coat or plane. In this way the dark plane may be deflected in any direction, and by a very strong blast the coat rendered imperceptible. A body at the temperature of the air is probably destitute of a visible coat. On black surfaces it is hard to establish this point, as the light necessary for their examination warms them

so rapidly. The careful study of a glass plate, or bright metal surface, however, reveals the fact that the coat is absent when first illuminated; its rapid formation may be seen. The coat appears to reach its maximum thickness very rapidly. At first we examined a body at the neutral temperature as it passed gradually from a temperature lower than that of the surrounding air to a higher. This method of examination led one of us to suppose, and to imply in the letter to 'Nature' (26 July, 1883, vol. xxviii. p. 297), that the coat was present on all bodies, whether warmer, colder, or at the same temperature as the surrounding air. But this method of examination is unreliable, as may be seen from the following.

A rod of carbon is cooled to $-21°$ C. in a test-tube surrounded by ice and salt. The rod is quickly transferred to the glass box (fig. 5), smoke introduced, and the beam, filtered through water, turned on. The temperature being low, the first thing seen is a bright down-stream of particles. Very soon a dark streak in the middle of this appears, which dark streak widens and becomes a sharp and well-defined down-streaming dark plane, edged at first with the bright one, but later becoming free from it. The convection-currents are still rapid, and the only portion of a coat visible is the thickened base of the (inverted) plane. This incipient coat is soon traceable further.

This incipient coat becomes visible further round the rod, until the lower half is provided with a thin black coat. Soon the downward convection-currents cease, the plane loses its definiteness, and becomes confused with its thickening base and the coat itself, until the ill-defined mass of dust-free air turns around the rod of carbon, thus giving rise to a hazy ill-defined adventitious coat, which, if removed, would not be able to re-form at this temperature. The currents then begin to ascend, and this adventitious coat gives place to the true coat and sharp plane, already described as existing when carbon is warm. The appearance presented by the cold but warming rod seems capable of some modification, in consequence probably of variations in the rapidity with which the carbon is heated, and of its being an imperfect conductor. Thus occasionally the dark ill-defined dust-free space which exists at the bottom of the rod, just as the process

of inversion commences, turns round towards the lamp, and may be succeeded by the ordinary black coat and a plane on that side, whilst, so far as has been ascertained, the coat may be partially wanting on the side remote from the lamp. Under these conditions one half of the base of the plane is more or less absent. This constitutes the unilateral plane shown in fig. 3.

3. *Experiments made with different Substances.*

Various substances have been examined in dusty air, such as copper, iron, zinc, carbon, glass, mica, selenite, vitreous selenium, Iceland spar, potash, rock-salt, bismuth, blotting-paper, black paper, white paper, chalk, polished silver, &c.; carbon moistened with various liquids; water, ice.

It may be stated, in a general way, that all these substances, when examined in the beam of the electric lamp, exhibit a remarkable similarity in their action towards the dust in the air, although they no doubt exhibit differences in the thicknesses of the coats. This is almost certainly exclusively dependent upon the temperature to which they are raised, and it is very probable that a thin layer of black camphor-smoke would make it difficult to distinguish between them. Ordinary wood-charcoal yields a coat remarkable for its thickness, in which respect it resembles electric-light carbon. Black solids and bad conductors favour the action by their high surface-temperature. Repeated heating to redness and cooling *in vacuo* certainly produces little, if any, diminution in the thickness of the coat of electric-light carbon.

Porous solids moistened with more or less volatile liquids yield wide coats, and sometimes downward black planes. In the dust-laden atmosphere they have been observed to give rise to a partial double coat and two planes. When moistened with two liquids of different boiling-points, no two distinct planes and coats were obtainable, although the boundaries of the observed coat and plane may not have been as distinct as in the case of a solid in merely smoke-laden air.

Liquid surfaces are capable of giving rise to dark planes of great distinctness in the dusty air above them. This may be well shown by attaching a thin horizontal platinum wire to

the ends of two copper wires passing through the tube of a funnel, rendered water-tight by means of sealing-wax. Water is then introduced into the funnel until it is about to overflow. Above the surface of the water is the smoke-laden atmosphere of the box. The platinum wire just beneath the liquid surface is then warmed by means of a feeble current, and the light turned on: a clear and distinct plane can then be distinguished rising from the thin line of warmed liquid. If the whole liquid surface be allowed to get warm a thick dark coat is visible above it, and a large columnar dark plane rises from the centre.

Our experience with mica was interesting. The first small sheet which we tried was placed horizontally in the box with magnesic-oxide smoke. The light revealed little growing bushes or trees of aggregating magnesic-oxide particles. The mica was evidently electrical. Rubbed sealing-wax acted in a similar way, but so did an unrubbed rhomb of Iceland spar to a slight extent. We at first thought that the mica might be photo-electric like certain crystals observed by Hankel. All pieces of mica do not behave in this way. Pressure between the finger and thumb, writing upon the surface with a blunt point of cork, or with a pencil on a piece of paper laid upon the mica, all seem capable of producing the deposition of the white dust upon these portions, but in a lesser degree. There would seem to be a possible relation between these effects and the "Hauchbilder" of Moser (Wüllner, *Physik*, Bd. i. S. 416, Dritte Auflage). Perhaps the removal of moisture-condensing nuclei may not be without influence upon the correctness of his explanation. The action in the case of mica would not, however, seem to be wholly, nor necessarily, electrical, as the following observation would tend to show. A Chladni brass plate was casually used by the assistant as a desk upon which to support a piece of paper upon which he was writing with a pencil. Upon subsequently bowing the plate, after dusting Lycopodium upon it, the writing became legible upon the surface of the brass. This action of mica is not, so far as we have observed, produced by simple heating. With a slab of tourmaline cut perpendicular to the axis from a crystal belonging to Prof. N. Story Maskelyne, the effects of heating, cooling, and re-heating

this pyroelectric crystal by the beam of light were very distinctly shown. We believe that some such simple and delicate method as that here indicated may be of use in the investigation and demonstration of such actions.

4. *Experiments made to ascertain the Effect of Pressure in the Smoky Medium.*

The apparatus for this purpose has already been referred to; it is shown in fig. 9. It could be connected with a water air-pump, which reduced the pressure to a few centimetres of mercury, or with a large iron cylinder, in which the air was condensed by pressure of the water-main. The open mercury manometer indicated a pressure of $4\frac{1}{2}$ atmospheres (in excess of the atmospheric pressure).

Tobacco smoke was introduced by slight exhaustion, and the small rod of electric-light carbon, shown in section in fig. 9, was heated by the electric-light beam being converged upon it. Under an absolute pressure of $5\frac{1}{2}$ atmospheres the convection-currents became very slow, the black plane wavy and unsteady, whilst the coat was so thin that only with the aid of the hand-lens could it be traced with certainty on the side of the carbon directly exposed to the heat of the lamp. The plane, moreover, was incomplete at its base—unilateral, a further indication that the coat was probably wanting on the colder or shadow side of the carbon-rod. Under a pressure of about 6 centim. the coat was wider than in air at the barometric pressure. The dependence of the thickness of the dust-free coat upon pressure was well marked; and already some preliminary numerical determinations have been made, which we hope will shortly enable us to determine the law of the dependence of the coat upon pressure with some degree of accuracy. The following figures for air serve to show that the thickness of the coat varies at different pressures. They were made with the apparatus shown in fig. 8.

Pressure of air, in centim. of Hg.	Thickness of coat, in centim.
75·9	·013
20·8	·027
10·9	·054
3·9	·053

The variation with pressure of course suggests ideas connected with the free path of molecules at the surface of the body; but the thickness of the coat is far greater than that ordinarily reckoned as the length of free path corresponding to the pressure.

The effects of electrification on the dust-free coats and the examination of electrical actions in dusty air generally were preliminarily noted in 'Nature,' July 1883, but the investigation of them is yet incomplete.

5. *Experiments with different kinds of Smoke.*

We have endeavoured to ascertain whether any of the phenomena obviously depend on a drying or on a combustion of the smoke-particles. Bearing in mind the very wide coat which surrounds an incandescent platinum wire, it seemed to us, even in this case, improbable that dust-particles were often burnt up by it, although we are well aware of the ease with which finely divided matter will oxidize. Chiefly with this object in view, we repeated an experiment adduced by Tyndall to show that combustion was a *vera causa* of the dust-free up-streaming air, but we used the incombustible and non-volatile particles of MgO. For this purpose we have employed a platinum wire, fig. 6, enclosed in the box fig. 5, and ignited by a current from a battery. The appearances are unchanged, and the air of the box rapidly clears; the white smoke being deposited upon the cold sides of the box. When volatile particles, such as ammonic chloride, are used, a thicker coat is observed at high temperatures than with magnesic oxide. This is probably in consequence of volatilization; for at low temperatures, such as 100°, no such difference is noticeable. When a rod of camphor is examined in air laden with magnesic-oxide particles, a dust-free coat and plane may be distinguished. But as the camphor gets gradually heated it volatilizes and makes the coat on the side near the lamp far wider than elsewhere. Hence it would appear that the volatilization of the camphor keeps back the dust-particles beyond the normal distance. The camphor-particles can be distinguished amidst the particles of MgO on the boundary of the coat and plane near the light, on account of their crystalline sparkling nature.

What influence the size of the dust-particles exerts upon the thickness of the coat has not yet been fully made out experimentally.

6. *Experiments on Bodies which themselves give off Smoke.*

We early noticed that a distinction had to be drawn between smoke given off from the warm body itself and smoke which belonged to the air external to the body. The latter seemed to be repelled from the body; the former seemed to come into contact with its surface.

If tobacco-smoke is blown on to an illuminated strip of common window-glass, the smoke nearest to its surface hovers tenaciously about long after the more distant smoke has disappeared; but a visible dark coat is still found to separate the dust from the surface.

There are many ways of getting a body covered by a layer of smoke. One way is to blow it on to its surface through a capillary tube ; another way is to pass the copper rod through the axis of a narrow glass tube fixed in a cork at the end of the box, so that the rod projects into the experimental glass box (fig. 5), and blow smoke through the tube. The smoke rushes along the surface of the rod, forming a sort of tube, but is gently carried away as the rod gets warmer, in the manner described, showing itself all the time as a white coat and plane inside the dark coat and plane which separate and keep back the general dust in the air of the box.

Another way is to let smoke ooze through holes in the sides of a tube upon which the light is allowed to fall. But the easiest plan is to use a substance which generates its own smoke, such as phosphorus. A common stick of phosphorus in ordinary air produces copious white fumes, which, being heavy, descend, and when illuminated a white coat and well-marked descending white plane are seen. The white coat is very thick, and hence probably it is that no portion of the black coat or plane is to be seen. If the air in the box is gradually dried, however, oxidation of the phosphorus so far diminishes as to enable an observation to be made upon the phosphorus in the particle-laden air before it again begins to emit smoke. This reveals the phosphorus surrounded by the dark coat and plane. But as the temperature rises the oxi-

dation proceeds more rapidly, and the black coat and plane gradually become more obscure, as the smoke coming from the surface of the solid enters them.

7. *Experiments on the Settling of Dust.*

Black paper-sheets and metal plates blackened with camphor smoke have been arranged in various positions in a box full of the white smoke of magnesia. It is observed that those which are warm receive barely any deposit of dust, notwithstanding the large volume of air which has passed near them. This is not due to the deposit being blown away.

Of surfaces at the temperature of the air those seem to collect most dust over which the air is the most stagnant; the dust has time to settle on flat level surfaces by common gravitation unless the motion of the air over them is too rapid. Cold surfaces collect a large quantity of smoke, and become coated over with a thick white deposit, as if it were attracted by the solid. Among cold surfaces rank all those walls of the box which are not directly warmed by the beam of light. If warm smoke is blown through a tube on to such a surface it adheres very largely, giving a thick local deposit opposite the tube.

Impressions showing the dark plane can be obtained by placing glass plates near warm solids in dust-laden air; and we have copied such impressions of the dark coat and plane on to sensitive paper by ordinary photographic printing *.

8. *Experiments in different Media.*

(a) *Gases.*—By far the larger number of our experiments have been conducted in common air, but we considered it highly important to observe the behaviour of various dry gases of very different density from air, and also to see whether any phenomena of the same kind could be detected in liquids.

With the apparatus shown in fig. 8, and already described, we have ascertained that hydrogen gives a much thicker coat, and carbonic anhydride a thinner coat, than is obtained in air, under conditions which we believe to be fairly comparable.

* Mr. John Aitken would also appear, from an abstract in 'Nature' (31st Jan.), to have obtained these records upon glass. It is singular how closely many of his experiments run with ours.

The figures in the following Table will serve to indicate roughly the differences which have been observed :—

	Temp. of solid.	Temp. of gas in observing vessel.	Thickness of coat in centim.
Hydrogen	41°	17°	·033
Ammonia	65	26	·013 (?)
Air	82	14	·020
Carbonic Acid	63	20	·011

The first column gives the temperature indicated by the thermometer upon the blackened bulb of which the coats were measured. The third column gives the measurements made at the middle of the bulb on the side near the light. In the middle column the temperature of the gas in the experimental chamber, as indicated by the second thermometer, is given.

The quantitative portions of this paper are as yet incomplete, and we consider the above figures only as indicating the differences provisionally. Some gases, moreover, appear to vary considerably in their action towards the camphor-smoked surface; and, in short, there are obscure conditions which affect quantitative results in a way which we do not yet fully understand.

(b) *Liquids.*—The outline of the method employed for the study of the appearances observed in liquids holding fine particles in suspension has already been described (fig. 10). The box is best filled with cold water from which the air has been expelled by recent boiling, and to which a little ferric oxide has been added, to render it turbid. When the beam of the lamp is now converged on to the surface of the empty brass steam-tube, there is usually to be seen a slightly dust-free space of liquid beneath the tube. Under the influence of the beam this becomes thinner, and two dust-free planes rise from each side of the tube, ultimately meeting upon the top of the rod, and often at first enclosing particles between them, as occasionally observed in gases. A dust-free black coat and plane are then seen, distinct but rather thin. This seems

to be a permanent state so long as these conditions are preserved unaltered. The convection-currents are fairly well developed, and the velocity-distribution resembles that described for a warm solid in dusty air. If now a current of steam be sent through the brass tube, the convection-currents become more rapid, and the coat so thin that we are not certain of its existence. Moreover, the distinctly dust-free black plane is no longer quite certainly there, although a casual observation would probably lead to the conclusion that it was. A long fine up-streaming layer of liquid, differing from the surrounding liquid, *is* distinctly visible, and a very similar result is obtained from a platinum wire strongly heated by the passage of an electric current. There is no doubt that liquids holding small particles in suspension are capable of giving rise to appearances similar to those described in gases, and it seems that in a liquid an increase of temperature occasions a less rapid increase in the thickness of the coat than is the case with a gas; or, in other words, the coat is thinner at high temperatures such as 100° C. than it is at a lower because the convection-currents have more power to destroy it.

Examination of the Hypothetical Explanations already suggested.

At the commencement of the paper are given all the provisional explanations which have been hitherto suggested as accounting for the dark plane so far as we are aware. The existence of the coat renders several of them improbable, but they appear capable of direct disproof, thus :—

Any centrifugal-force hypothesis is negatived by the points of inflexion on the stream-lines of fig. 1, and more conclusively by fig. 2, where the white space between the parallel curved lines represents in section a hemicylinder of sheet-copper with its black coats. The convection of the stream-lines is here such as to whirl the particles into the coat rather than out of it. The evaporation and combustion hypotheses are disproved by the use of a non-volatile and incombustible smoke like magnesic oxide; also by the phenomena being observable in liquids.

There remains only the "distribution" hypothesis of

Tyndall (originally given Proc. Roy. Inst. 1870, vol. vi. p. 3), viz. that the dust lags a little behind the air as it starts off in a convection-current. It is not so easy to negative this, and in fact in some form or other it is certainly true that the dust does get filtered out of the air, being all made to keep outside the dark coat, while some of the air passes in. The only question is, Why does the dust get driven out of the air in this way? what is there near the surface of a warm body which keeps back the dust? A mere lagging behind of the dust particles by reason of their extra inertia seems to us a quite insufficient statement. The first formation of a coat on the surface of a solid before the convection-currents have started, and the case of the interior of a glass tube, may be adduced as negativing any purely convective explanation: the dust-free coat is seen not only on the outside of a tube where convection-currents are in full swing, but it is well marked also as an internal lining of the tube where the air and dust are both stagnant. The internal lining, though thick, is of irregular thickness, and has not the sharp boundary of the outside coat. The universal effect of convection-currents is to sharpen the boundary of the coat, but to thin it down. This is, indeed, a very important fact, and leads straight to the conclusion that the formation of the dark coat is an operation which requires time, though only a very short time, and that it is possible to carry off a great part of the coat more quickly than it can be renewed.

Suggested Explanations.

The provisional explanations which have occurred to us as possible during the course of the investigation have been very numerous, and have been mostly one after another discarded, and only one or two are at all worthy of being here mentioned.

One notion which occurred pretty early in the experiment, if it had held its ground, would have reduced the whole thing to the merest mechanical phenomenon, just as Dr. Tyndall's original ingenious explanation (to which, by the way, we were more than once tempted to revert) or Lord Rayleigh's centrifugal-force notion would, if true, have deprived the dark spaces of most of their physical interest. It was in the belief

that something deeper than common mechanical principle was at the root of the appearances that we were originally tempted to examine them so closely; and we are not sorry that the barbarous simplicity of the notion now to be mentioned has failed to explain them, though at the same time we still believe it modifies the effects, and that sometimes notably. We call it the settling hypothesis, and it may be stated thus :—

Dust falls through fluids at a pace depending on the size of the particle and the viscosity of the fluid, but the relative settling velocity is not affected by the motion of the fluid, whether horizontal or vertical. We may regard all dust as constantly settling relatively to the fluid in which it is.

By so settling, it may leave dust-free spaces.

Under a horizontal plate, therefore, the dust might settle, and leave a clear dust-free coat; so it might also perhaps immediately under the middle of a round rod; and it is just conceivable that air from this clear space might get partially carried up and round the rod by convection-currents no faster than fresh settling kept on renewing it below.

It is undeniable that gravitation must assist the formation of coats on the under surface of plates, and must do its best to spoil any coat on the top. The action of gravitation in this way is described in various parts of the paper. It is impossible, however, for any one witnessing a really good dark coat under a warm rod, and seeing the rushing-up of the dust against the dust-free coat, to go away with the notion that gravitative settling has produced that sharp, definite, and thick coat against which the up-rushing dust-particles almost seem to rebound. The idea is irresistibly suggested of something keeping them forcibly off.

Moreover, suppose a plate to be originally at the temperature of the air: it has no coat. Turn on a powerful light: a coat instantly forms, and rapidly thickens under the eye before any convection has time to begin. It looks just as if the dust were driven out from its surface by some action which depends upon the temperature.

Again and again have we been driven to the notion of a molecular bombardment carried on from the surface of the body, by which the dust-particles are driven away like the

vanes in Mr. Crookes's radiometer, or like drops in the spheroidal condition. But we were unable to take up precisely this position because of the enormous thickness of the dust-free coat as compared with the mean free path of an air-molecule at the ordinary pressure.

We then tried to develop a notion of an *extreme* free path: we thought whether the dust-particles might not be so easily moved that the impact of even a few molecules on their surface would be sufficient to drive them back; and we tried to imagine that, though the great bulk of the flying molecules only shoot a distance from the surface reckoned in hundred thousandths of a millimetre, yet a few, say an odd million or so per second, might escape collision for a distance of even half a millimetre, and that these might drive back the dust. The difficulty here would be to see why the boundary of dust and no-dust is as sharp as it is, though we tried to imagine that this was caused by the convection-currents, a definite amount of bombardment being necessary to drive back the dust when carried towards the surface at a given rate, and this definite bombardment being found at a definite distance from the surface. We were now fairly landed, by the observations we had made, on some molecular bombardment hypothesis or other, while before this we had been searching among all kinds of mechanical, electrical, and other notions of a vaguer kind. The necessity for some kind of bombardment explanation being thrust upon us, it was not long before we perceived that no considerations of free path were necessary, but that a differential bombardment on the two sides of a dust-particle would be sufficient to drive it back. In other words, a dust-particle would move from a region of greater pressure to a region of less, being urged by a force depending on the size of the particle and the pressure-slope.

Consider a warm flat vertical surface of copper. Adhering to the surface is a layer of condensed gases with very different properties from ordinary air, which forms part and parcel of the body, and constitutes a transition between genuine air on the one side and unmistakable copper on the other. This layer, or " Bunsen-schicht," is warmed directly by contact with the solid, and by molecular diffusion it warms the next outer

layer, which, sending out quick-flying molecules, warms the next, and so on. Ultimately a stationary state is reached, and the temperature must fall continuously as we recede from the surface, according to some exponential law when convection-currents are permitted, but otherwise linearly.

The air-film next the solid is known to cling tightly to the solid, and not to be quickly removed. Its adherence need not, indeed, be one relating to individual molecules; it may be only a statistical one, but its renewal is known to be such a slow process that it is very unlikely that it appreciably streams upward even when fairly hot. The layer outside this streams upward a little, being retarded by viscosity or molecular diffusion as it shears itself over the inside layer. It in its turn retards the next, and so on, the velocity of upstreaming increasing as we go out from the surface. But this increase of velocity of successive layers only goes on to a certain limit; it soon reaches a maximum, because the temperature of the successive layers is rapidly decreasing, and thus they have less tendency to rise although there is now very little to prevent them, until at a distance from the solid the air is at rest.

There is, then, in all convection-currents from a vertical surface a layer whose velocity is a maximum; on either side of it the velocity is diminishing—on the cool side because the temperature gets less and less; on the hot side, in spite of the high temperature, because of the viscosity. The maximum-velocity layer marks the position of compromise between the lightening due to temperature and the resistance due to the neighbourhood of the solid.

The fact that the convective velocity increases and reaches a maximum as you recede from the solid, combined with the fact that the temperature constantly diminishes as you go in the same direction, seems to us to have an important bearing on the production of the dust-free space by molecular bombardment in a way we will endeavour to explain.

A mere steady fall or gradient of temperature as one recedes from a body will not of itself produce the differential bombardment necessary to keep back the dust-particles. For though it is true that the air is hotter on the side next the body than it is on the far side, this excess of temperature does not result

in an excess of pressure but in a defect of density. The air near a warm body is less dense than the air outside, but it is not at a greater pressure; at least, if it is, it is a new phenomenon, and one not to be expected from any known action of a solid upon gas, except, of course, within the extremely minute range of the molecular forces estimated by Quincke at the five-millionth of a millimetre. The warm gas therefore would produce no greater bombardment on the one side of the particles than the cold gas produces on the other; for though each molecule hits harder and hits oftener, there are fewer molecules to hit; and the one effect compensates the other as soon as a stationary condition as to distribution of temperature has been attained. Not before; for so long as the air near a solid is getting warmer, there is a more rapid diffusion of molecules outward than there is inward, and till equilibrium is reached there will certainly be a pressure outwards from the body sufficient to drive the dust back.

If the air near a solid can be kept stagnant while the body is warmed, the dust in it ought to be driven back at first by the outward heat-diffusion; but as soon as a stationary temperature-condition has been attained, this bombardment ought to cease, and permit the dust to slowly settle back again if it likes. This is much the case of the interior of a glass tube, and, we believe, explains the quick formation of a coat when the light is first turned on to any surface, and also the reason why such coats in stagnant air, though broad, are hazy and indefinite in outline.

But suppose the air near the body is rapidly changed (in the case of the tube by blowing through it, or by convection-currents in ordinary cases), the stationary state never is reached, the air outside the body is always getting warmed, and the bombardment may always continue. The function of the convection-currents, then, is to prevent the arrival of the stationary state, and so to perpetuate the dust-repelling bombardment, at the same time that they sweep off some of the already cleansed air and bring up dusty air at such a rate that the bombardment has as much as it can do to keep a layer clear. The coat therefore tends to become thinner when convection-currents increase; and if currents are unfairly produced (as by blowing), the coat may be swept away faster than it can

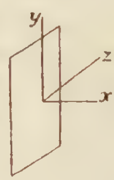
be renewed, and so wholly disappear. But if excess of temperature only is the cause of the currents, then the same cause which strengthens them also assists the bombardment, and accordingly the coat is not swept off by such currents, but may even become thicker as the temperature rises. At the same time it is not to be expected that small differences of temperature will much affect the coat, because of the compensation action already explained. Moreover, we have at present no theoretical guarantee that a rise of temperature need thicken the coat; it might thin it in some media. All we can say is, that convection-currents have a double action, both helping the formation, and causing the removal, of the coat.

But now, how does the fact of an increasing upward velocity of the air as we go through successive layers from the surface of a body account for the differential bombardment or greater internal pressure necessary to account for the driving back of the dust? Without technicalities, we answer this question as follows:—Consider a vertical flat plate at a higher temperature than the air. We grant that the total pressure of the air is the same near a warm solid as it is anywhere else; that is, the average mean square of molecular velocity is simply proportional to the temperature; but then the average mean square of velocity can be resolved into three components—normal to the surface, parallel to the surface and horizontal, and parallel to the surface and vertical. If the air were at rest, all three components would be equal; but in any given layer the air is not at rest, it is up-streaming: consequently the vertical component of velocity is greater than the average, and so some other component must be less. The horizontal component of velocity parallel to the surface we naturally assume to be simply the average. The component normal to the surface must therefore be less than the average. In other words, though the total pressure is the same in all directions, its vertical component in the case of up- or down-streaming air is greater, and hence the normal component is less; and the amount by which the normal component of the pressure is less than the average at any given distance from the body depends upon the velocity of the convection-current at that place.

But the current-velocity increases as we go from the body

to a maximum, and then decreases: consequently the normal component of the pressure, starting from its average value close to the surface, decreases as far as the maximum-velocity layer, where it reaches a minimum, and then increases. It is this differential normal pressure from the surface which the dust-particles feel, and they are driven back towards the maximum-velocity layer. They may not, indeed, be driven quite to it, because there must be a compromise between the rate at which dust is carried past the surface and the distance to which the differential bombardment has time to drive them through the air.

It is easy to see that any differential bombardment will be simply proportional to the volume of the particles, provided their thickness is small; hence the behaviour of big particles relatively to small ones will be like their settling behaviour under gravitation.



Putting the matter in symbols, let us call the plane of the solid yz, y being vertical; and let u, v, w be the three component velocities along x, y, z respectively, and T the absolute temperature; and let $=$ signify proportion only when convenient: then

$$u^2+v^2+w^2=3\,\mathrm{T}.$$

But at a distance x from the surface let the convection up-streaming velocity be ϕ, and let the velocity along z be the average: then

$$w^2=\mathrm{T},$$

and $$v^2=w^2+\phi^2;$$

so $$u^2=\mathrm{T}-\phi^2.$$

Hence u decreases as ϕ increases, and there is accordingly a bombardment up towards the maximum-velocity layer from either side.

Calling the three components of the pressure $p_x, p_y\ p_z$, it can be shown that

$$p_x=\rho u^2,$$

$$p_y=\rho v^2,$$

and $p_z=\rho w^2=p$, the proper pressure of the gas.

Hence $$p_x=\rho(\mathrm{T}-\phi^2)$$

$$=p-\rho\phi^2=p\left(1-\frac{\phi^2}{\mathrm{T}}\right);$$

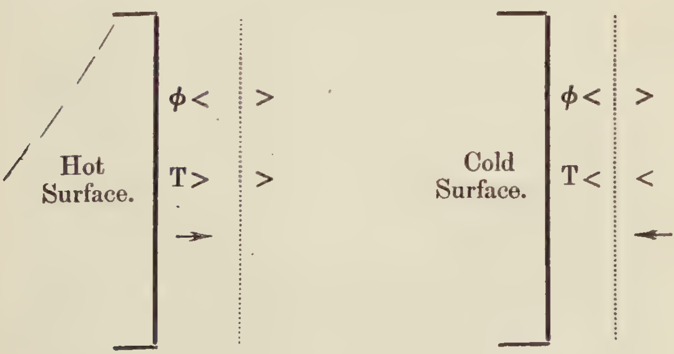
and $\frac{dp_x}{dx}$, which determines the intensity of the differential molecular bombardment per unit distance along the normal to the surface, is proportional to the original pressure and to the rate of change of $\frac{\phi^2}{T}$.

Remember that ϕ is the up-streaming velocity, and T the temperature, of the different layers. In a region where ϕ is increasing and T is decreasing, the differential bombardment is considerable, and it acts in the direction of increasing ϕ. In a region where ϕ and T are both increasing or both decreasing the bombardment is likely to be feeble, and may be *nil* if ϕ varies with the root of T.

Now outside a warm plate we have near the surface ϕ increasing and T decreasing as you go outwards : consequently here is an outward bombardment. Further on both are decreasing, so the bombardment is feeble or *nil*, and one cannot say without further consideration which way, if any, it ought to act.

Close to the surface of a cold body proceeding outwards ϕ is increasing, but T is also increasing : consequently there is here feeble bombardment. Further on ϕ is decreasing and T still increasing ; consequently here is a bombardment inward towards the body.

Representing these things diagrammatically, the arrow represents the direction of the bombardment when it is decided, the dotted line the maximum-velocity layers :—



Now, the conductivity of a gas for heat, and the viscosity which resists its own motion, are each of the nature of a diffusion or interchange of molecules; and, as shown in Maxwell's 'Heat,' the three things—the diffusivity, the conductivity, and the effective (or "kinematic") viscosity, are all proportional to one another, and vary directly with the square of the absolute temperature, and inversely as the pressure.

Returning to the warm solid, with its up-streaming air-currents and maximum-velocity layer towards which dust is bombarded from the surface, it is evident that a rise in the temperature of the solid would throw this layer further out from its surface (and so thicken the dust-free coat), both because the conductivity of the gas improves, and because its viscosity near the surface increases with temperature. A fall of temperature, on the other hand, would throw the maximum-velocity layer inwards, until, when the solid was colder than the air, the maximum down-streaming layer might be very close to the surface, because its viscosity would be least there, and because the conductivity of the gas would then be so poor. (N.B. We never assume that the dusty air is warmed by *radiation* from the solid, because the far more powerful electric-light radiation produces no particular effect on the dust except in the neighbourhood of bodies.) The effect of an increased pressure would be to diminish both the conductivity and the viscosity in the same proportion; and so for both reasons it would throw the maximum-velocity layer inwards, in fact it would act like cold. Rarefaction of the air would act like an increase of temperature.

Hydrogen has a higher conductivity and viscosity than air, consequently its maximum-velocity layer should be further from the surface of the warm solid exciting the currents. Carbonic acid, on the other hand, should act in the opposite direction.

Very little is known about the properties of liquids, but their kinematic viscosity is known to be less than that of gases, and it is supposed to decrease with a rise of temperature (Wüllner, Bd. I. S. 326, Flow of liquids through tubes at different temperatures). Hence it may be guessed that the maximum-velocity layer in liquids should be much nearer the surface of the solid causing the currents than in gases, and that

a rise of temperature would bring it still nearer. On the other hand, it may be expected that cold would drive it further off the solid, so that any phenomena connected with this layer in liquids might be better developed in descending cold currents than in ascending warm ones.

In conclusion, the writers wish to call attention to a paper read to the Royal Society of Edinburgh, by Mr. John Aitken, on the 21st of January this year, and of which an abstract appeared in 'Nature' of the 31st January last. Judging from the abstract, it would appear that Mr. Aitken, so well known for his previous researches on dust-particles as nuclei, has been travelling over much of the same ground as we have, and that he has arrived on the whole at similar conclusions.

University College, Liverpool.

II. *On some Experiments illustrating an Explanation of* Hall's *Phenomenon. By* Shelford Bidwell, *M.A., LL.B.**

[Plate II.]

In a recent communication to the Royal Society I attempted to show that the apparent action of a magnet upon an electric current (well known as Hall's Phenomenon) might be completely explained by the operation of mechanical strain and certain Peltier effects.

My experimental acquaintance with the subject began in July 1883, and I have since devoted a considerable amount of attention to it. But the paper referred to contained only a bald and simple statement of the results at which I finally arrived, little or nothing being added by way of comment or explanation. To my mind, saturated as it was with the matter, it appeared that such a statement afforded in itself overwhelming evidence in favour of the views which I had been led to adopt; and I believed that when once set forth, however briefly, they could not fail to meet with immediate acceptance. But Hall's own theory of the phenomenon has for some years been universally recognized as interpreting a well-established law of nature. It seemed to be in complete harmony with the views of Clerk Maxwell as to the existence

* Read March 8, 1884.

of a rotatory coefficient of resistance: it has provided a field for investigation to mathematicians of the highest eminence; and it has derived additional interest and importance from the fact that it was believed to point to an intimate relation between electricity and light. Under the circumstances, it is reasonable that such a theory should not be abandoned without the exercise of very considerable caution.

I propose therefore in this paper to treat the subject in greater detail than at first seemed necessary. Attention will be directed to facts and arguments which in my former paper were left altogether unnoticed; and those points which, as I have gathered from conversation and correspondence, seem to present the greatest difficulty, will be considered at some length. With regard to the experiments upon which I rely, such of them as are new are of a very simple character, and can easily be repeated by any one possessing a reflecting galvanometer. They are of course of a somewhat delicate nature, and more suitable for the laboratory than for public demonstration; but I shall be bold enough to attempt the performance of most of them in the presence of the members of the Physical Society.

Mr. Hall's first paper on the subject appeared in Silliman's Journal in 1879, and is reproduced in the 'Philosophical Magazine' for March 1880. After stating that his object was to prove that the current of electricity in a fixed conductor is acted upon by a magnet, he describes the following experiment:—A strip of gold-leaf, forming part of a circuit, was placed between the poles of an electromagnet, cutting the lines of force perpendicularly. The terminals of a galvanometer were applied to opposite edges of the gold-leaf, and moved about until two equipotential points were found. The magnet was then excited, and a galvanometer-deflection immediately occurred. This deflection was too large to be attributed to the direct action of the magnet upon the galvanometer-needle. It was permanent, and so not due to induction. It was reversed when the magnet was reversed. "In short," says Mr. Hall, "the phenomena observed were just such as we should expect to see if the electric current were pressed, but not moved, toward one side of the conductor. In regard to the direction of this pressure, as dependent on the direction of

the current in the gold-leaf and the direction of the lines of magnetic force, the following statement may be made :—If we regard an electric current as a single stream flowing from the carbon pole of the battery through the circuit to the zinc pole, the phenomena indicate that two currents in the same direction tend to repel each other. But if we regard the current as a stream flowing from negative to positive, then the phenomena indicate that two currents in the same direction attract each other." The meaning of this is, as appears from subsequent papers, that the direction of the transverse current across the gold-leaf is opposite to that in which the gold-leaf itself would move across the lines of force if it were free to do so. If the two points to which the galvanometer is connected are joined by an equipotential line across the gold-leaf, the effect caused by magnetization may be expressed by saying that the equipotential line is rotated in a direction *opposite* to that of the current circulating in the coils of the electromagnet.

A second paper was published in the 'Philosophical Magazine' for November 1880, in which were described experiments made with other metals in addition to gold. With silver, nickel, tin, and platinum the direction of the transverse effect was the same as with gold, though it differed in degree in the case of every metal. But with iron the direction of the transverse current was found to be reversed; a fact which Mr. Hall was naturally inclined to attribute to the magnetic properties of that metal, though he was at a loss to account for the difference between its behaviour and that of nickel, which is also strongly magnetic. In iron, therefore, the equipotential lines are rotated in the *same* direction as that of the current circulating in the coils of the electromagnet.

In a third paper, published in the 'Philosophical Magazine' for September 1881, the author says that the direction of the transverse electromotive force in the magnetic metal cobalt was the same as in iron. In this paper it is stated that $\frac{E'}{V}$ is probably a constant for any given metal; E′ being the transverse electromotive force per centimetre of the width of the strip, and V the direct current divided by the section of the conductor. This quantity $\frac{E'}{V}$ is called, in accordance with a

suggestion of Dr. Hopkinson, the "rotational coefficient" of the metal. The direction of the transverse effect or sign of the rotational coefficient is in iron and metals which behave like iron called +, because its direction is that which the conductor itself bearing the current would follow if free to move across the lines of magnetic force. In gold and similar metals the sign of the rotational coefficient is called −.

A fourth paper appeared in the 'Philosophical Magazine' for May 1883, entitled "Rotational Coefficients of various Metals," in which it is stated that aluminium, copper, and brass behave in the same manner as gold, that zinc behaves like iron, and that with lead no transverse effect whatever is produced.

Collecting Mr. Hall's various results, we find that, of the thirteen different metals which he has tested, three (iron, cobalt, and zinc) are classed as positive ; nine (gold, silver, tin, copper, brass, platinum, nickel, aluminium, and magnesium) are negative ; and one (lead) shows no signs of rotation.

I began a repetition of Hall's experiments with the object, if possible, of carrying them further, and especially of establishing in a satisfactory manner the existence of a connexion between the phenomena in question and the magnetic rotation of the plane of polarized light. My earlier experiments were made with iron, of which I happened to possess a suitable specimen in the form of foil ·019 millim. thick. The magnet used consisted of a bar of soft iron, 38 millim. in diameter, bent into horseshoe shape ; if pulled out straight its length would be about 50 centim. It was covered with about 600 turns of wire, 2 millim. in diameter; the pole-pieces used were 30 millim. square, with flat ends, and were generally fixed about 6 millim. apart. The magnet was excited by a battery of either seven Grove's or, more generally, of five 3-quart bichromate cells.

The arrangement of the apparatus is shown in fig. 1 (Plate II.), where P P′ is the strip of metal cemented to a plate of glass, and placed above one pole, S, of the electromagnet ; M is a section of the yoke of the electromagnet ; B is a battery of one or two cells, generating the current through the strip ; C a commutator to reverse the direction

of the current; B′ the electromagnet battery; C′ a commutator to reverse the magnetism; W W′ wires from equipotential points in the metallic strip to the galvanometer G; K a key in the galvanometer circuit.

The method of proceeding was as follows:—A current was passed from B through the strip P P′, and sliding-contacts on the opposite edges were adjusted until, when the key K was depressed, no deflection of the galvanometer occurred. The magnet was then excited by the battery B′, and on again depressing K the galvanometer was deflected. On reversing the direction of the current either from B or B′, by means of one of the commutators, an opposite deflection occurred when K was depressed. The galvanometer used was a reflecting instrument, of ·2 ohms resistance; it was placed at a distance of 4 metres from the scale, upon which it reflected the image of a very fine wire. The scale, key, and commutators were all within easy reach of the operator.

With this comparatively small apparatus there was at first some difficulty in producing a sensible Hall effect, and the greatest care had to be exercised to avoid disturbing influences. In addition to the possible sources of error mentioned by Mr. Hall, I was much troubled by one which he does not seem to have noticed, and which may probably account for the uncertain nature of some of his results. It was found that, though the galvanometer-deflection indicating the Hall effect was invariably reversed when the direction of the magnetizing current was reversed, it sometimes appeared to be altogether independent of the direction of the current passing through the strip. It occurred to me to excite the magnet without passing any current at all through the strip; and I found that when the key K was depressed a steady deflection of the galvanometer occurred, which was reversed on reversing the magnetizing current. That this effect was not due to the direct action of the magnet on the galvanometer-needle was proved by the fact that, when the key was raised, the deflection immediately ceased. It appeared at first sight as if the magnet had the power of generating a current in the strip of metal. But this, of course, was impossible if the magnetic force remained constant. I concluded therefore that it was an effect of induction caused by weakening of the magnet

in consequence of the running-down of the battery, or perhaps of the resistance of the wires being increased by the heating-effect of the current; but it was difficult to see how this could induce a current in the strip which was symmetrically situated with regard to the lines of force. Finally, I found that the effect was produced, not in the strip at all, but in the wire which passed from the strip between the arms of the electro-magnet to the galvanometer. When this wire was led out on one side of the yoke, the deflection produced was opposite to that which occurred when the wire was led out on the other side; and if the wire was turned half a dozen times round the magnet, the resultant galvanometer-deflection was so great that the spot of light disappeared from the scale. This was ultimately remedied by forming the wire in question into a loop embracing the yoke of the magnet, as shown at L L′ (fig. 1). From this loop an independent wire W′, attached to a sliding binding-screw D, was led to the galvanometer. It was found that the position of D could be so adjusted that, K being steadily depressed, the magnetism of the electro-magnet might be interrupted, or even reversed, without causing any material deflection of the spot of light even at the instant of the change. When so adjusted it was, of course, impossible that the comparatively small changes in the magnetic force, caused either by polarization of the battery or by the heating of the wires, could have any effect upon the galvanometer. The Hall effect now came out with great distinctness, the deflections to the right or left of the zero-point of the scale, caused by exciting the magnet, being generally as much as 15 or 20 centim.*

A number of experiments were now made with pieces of iron cut into various shapes, and occasionally with fine copper wires arranged in different forms, and the conviction gradually forced itself upon me that the effect in question was not due to any such action of the magnet upon the current as that which Mr. Hall supposed.

* That the magnetizing current used by Hall was not absolutely constant is evidenced by a statement in his paper in the 'Philosophical Magazine' for September 1881, that the readings of a tangent-galvanometer, placed in the circuit for a special purpose, "slowly decreased with the running-down of the current."

The possibility of a thermoelectric effect suggested itself at an early stage, the heat being derived from the current flowing through the strip, and the changes in direction being possibly produced by some effect of magnetization upon the thermoelectric properties of the metal. Such effects, I thought, could only occur at the points where the wires were attached which went to the galvanometer. Disks of bismuth and antimony were therefore inserted at the contacts of the wires and the strip, and their relative disposition varied in order that the thermoelectric effect, if any existed, might be exaggerated. But it was found that, whether the disks were of bismuth or of antimony, or of the two metals in various combinations, the extent of the galvanometer-deflections was unaffected, except in such a manner as could be perfectly accounted for by ordinary thermoelectric action. The idea of mechanical strain then occurred to me; and I may mention that this occurred to Mr. Hall, but only to be rejected by him as utterly inadequate to account for the phenomenon. I attached a string by means of sealing-wax across the middle of a thin sheet of iron, which was cemented to glass and connected with the battery and the galvanometer as usual. On pulling the string in a transverse direction I found unmistakable indications of a galvanometer-deflection in the same direction as if the sheet of iron had been acted upon, not by a mechanical pull, but by the electromagnetic force. The effect was small, but I have since greatly increased it by the following device:—A strip of thin iron was cemented between two thin slips of deal about 20 centim. long, forming a kind of sandwich. The sandwich was attached to a board by means of four screws, the distance between the middle screws being about 7 centim. The two ends of the iron were connected with a battery, and the middle points of its opposite edges with the galvanometer. On pressing with the finger the middle of one edge of the sandwich in a direction perpendicular to its length and in the plane of the metal, though no appreciable distortion was produced, the galvanometer was immediately deflected, the direction of the current from the strip to the galvanometer being always the same as the direction of the force. A similar sandwich was prepared in which platinum-foil was used instead of iron, and with this a transverse force produced

opposite deflections to those which occurred in the case of iron.

There is, of course, no doubt whatever, as to the real existence of a mechanical stress in a conductor through which a current is passing, and which is at right angles to the lines of magnetic force. Its effect may be rendered evident to the eye by a simple experiment, to which I attach no importance whatever except as affording a rather pretty illustration of a perfectly well-known law. A shallow glass cell is fitted with iron electrodes at its two ends, and is filled with mercury to a depth of 3 or 4 millim. When this cell is placed between the poles of an electromagnet and a current is passed through it, the mercury along the transverse middle line is drawn towards one side of the cell. Here it is heaped up, and, yielding to the force of gravitation, it runs along the two ends of the cell in the opposite direction, once more to be urged forwards across the lines of force. The result of a continuation of this process is the formation of two little whirlpools of mercury which rotate in opposite directions in the two halves of the cell. The same effect may be produced, but with less violence, if a solution of sulphate of zinc is used with zinc electrodes.

Having completely satisfied myself that mechanical strain was concerned in the production of Hall's phenomenon, it remained to ascertain the *modus operandi*. Thermoelectric action again occurred to me, and I remembered that Sir William Thomson had discovered that thermoelectric effects occurred in stretched and unstretched wires of the same material when brought into contact with each other. On referring to Thomson's paper, which was published in the 'Philosophical Transactions' for 1856, I found it stated that if a stretched copper wire is connected with an unstretched copper wire and the junction heated, a thermoelectric current will flow from the stretched to the unstretched wire through the hot junction ; while, if the wires are of iron, the direction of the current is from unstretched to stretched. There could be little doubt that effects of the same nature would occur in sheets of these metals; and I proceeded to consider the precise manner in which a sheet of metal conveying a current would be strained when placed between the poles of an electromagnet.

My first idea was that, since the magnetic force was strongest in the middle of the field, the lower half of the metallic sheet would be, on the whole, transversely compressed and the upper half extended. But I immediately saw that strains of this nature would not explain Hall's phenomenon, unless indeed it turned out to be the case that lateral compression and extension produced opposite thermoelectric effects to those caused by longitudinal compression and extension. The effect of lateral compression in an iron wire was noticed by Thomson in the paper referred to. He states that when an iron wire is laterally compressed, a current passes from uncompressed to compressed through the hot junction. He supposes therefore that since a lateral traction would probably produce the reverse effect of a lateral pressure, it would give rise to currents from stretched to free—the reverse of longitudinal traction. In making this experiment, Thomson used many turns of wire wound round a block of wood (to multiply the effect) and squeezed the whole in a hydraulic press. I attempted to repeat the experiment by a simple method and using only one wire; but no amount of pressure that I was able to produce resulted in any indication whatever of a current. I have since endeavoured to ascertain whether any appreciable current could be produced in a sheet of metal by lateral traction. The arrangement used is shown in Pl. II. fig. 2:—A B is a sheet of metallic foil; the shaded part D E was held between two fixed blocks of wood, and the shaded part C F between two blocks of wood which were attached to a lever. A heated glass rod was applied along C D, and on drawing the lever upwards in the direction of the arrow, a current immediately passed between the unstrained and the strained portions of the metal through the hot junction. Two metals only have been thus treated—zinc and iron; and the direction of the current was in both cases the same as if the traction had been longitudinal instead of lateral. In zinc the effect was very strong, in iron it was feeble, but not, I think, so feeble as to be doubtful.

On further consideration I came to the conclusion that no appreciable strains of the kind above referred to would be produced by the transverse force in the magnetic field. This force would be sensibly uniform across the whole of the middle portion of the strip, for the pole-pieces were flat and

within a very short distance of each other, and the strip did not extend across their whole width. Considering the analogy of a beam of wood, rigidly fixed at both ends, and acted upon in the middle by a force perpendicular to its length, I imagined that the transverse force would tend to make the metallic strip assume a distorted form of the same character as that indicated in fig. 3. As thus distorted, the strip may be mapped out into six districts, in three of which the metal is, on the whole, subjected to longitudinal traction, while in the other three it undergoes compression. This, of course, supposes that the metal, though cemented to glass, is capable of yielding to some extent to the force acting upon it. And since neither the metal itself, nor the glass, nor the cement which connects them, is absolutely rigid, this must necessarily be the case, though probably in a very small degree. The ends C, D of the strip, which are in a weaker part of the magnetic field, or even removed from its influence altogether, will not be acted upon in the same manner; and if the strip is sufficiently long they will remain absolutely at right angles to the middle line, as shown in the figure. Two points of contrary flexure will therefore exist on each of the edges; and the necessary consequence of this will be alternate regions of compression and extension, as indicated in the figure. It seems to me that, however thin the metal may be, so long as it is not altogether without cohesion and elasticity, the action of a transverse force across the middle will affect it in some such manner as that pointed out, to some degree which may be very small.

I have prepared a rough model to illustrate my idea of the nature of the distortion which the plate undergoes. A number of narrow strips of deal are attached at short intervals to a rectangular sheet of indiarubber: they are equal in length, and parallel to the shorter sides of the rectangle. The two strips at the ends of the sheet are nailed firmly to a board; otherwise the indiarubber is free. If the middle strip of wood is drawn or pressed in a direction transverse to the rectangle, the indiarubber is distorted and assumes the form of fig. 3; and the regions which are there marked as stretched and compressed appear very clearly, the strips being drawn apart in the former, and crowded together in the latter. The same kind of distortion would be produced, though to a

smaller extent, if the ends, instead of being rigidly fixed, were able to yield, more or less, to pressure or tension. But unless they were each attached to a single point, and were otherwise perfectly free (in which case the sheet would be longitudinally divided into two equal regions of compression and extension), some such distribution of strain as that indicated in fig. 3 must necessarily occur*.

Suppose now that a current is passing through the plate from C to D, and that A and B are two points on the opposite edges, which, when the metal is unstrained, are at the same potential. Then the ratio of the resistances between the points C and A, and the points A and D, is equal to that between the points C, B and the points B, D. And so far as mechanical strain alone is concerned, this equality will not be disturbed by placing the plate in a magnetic field, the strain produced being symmetrically distributed on both sides of the middle line. At all events, no strain could occur which would in itself affect the resistance of gold and iron in opposite ways, for the resistance of both is increased by extension, and (presumably) diminished by compression. But the currents from C to A and from B to D pass from regions which are compressed to regions which are stretched, while the opposite is the case with currents passing from C to B and from A to D. And here the thermoelectric effects come into play.

It has already been mentioned that a thermoelectric current will pass from a stretched copper wire to an unstretched copper wire in contact with it, if the junction is heated. From this it might be inferred, that a current would flow through the heated junction from an unstretched or free copper wire to a longitudinally compressed copper wire. And I have proved by actual experiment that this is the case. *A fortiori*, therefore, a current would pass through the heated junction from a stretched copper wire to a compressed copper wire. For similar reasons the current would, if the wires were of iron, flow in the opposite direction, from the compressed wire to the stretched wire, across the hot junction. And the

* It should be remarked that no account whatever is taken in the model of lateral compression and extension, which, as already stated, are probably very small in the case of the metallic sheets used in Hall's experiments.

same effects, so far as regards currents between stretched and unstretched portions, occur, as I have proved experimentally, if strips of foil are used instead of wires. If, therefore, a battery-current is passed from a stretched portion of a wire or foil to a compressed portion, heat will (according to the laws of the Peltier effect) be absorbed at the junction if the metal is copper, and will be developed at the junction if the metal is iron. In passing from compressed to stretched regions, the converse effects will occur.

Let us now imagine the metal plate to be divided into four equal regions A, B, C, D, as shown in fig. 4. Let a current pass through the plate from E to F, and let a force (produced electromagnetically or otherwise) be applied in the direction H G. First, suppose that the plate is of copper, then the current travelling from E to the line O G passes from a compressed to a stretched portion of the metal; heat will therefore be developed in the region A. Between the line O G and the point F, the current passes from a stretched to a compressed portion; heat will therefore be absorbed in the region B. For like reasons, heat will be absorbed in C and developed in D. The temperature of the copper plate will therefore not be uniform, the portions A and D being on the whole hotter than the portions B and C. But the resistance of a metal increases with its temperature. The resistance of A and D will therefore be greater, and the resistance of B and C smaller, than before the plate was strained. If therefore G H were originally an equipotential line, it is clear that it will be so no longer. An equipotential line through the point O will now be inclined to G H in the direction K L, as shown in the figure.

Supposing the plate to be of iron instead of copper, the Peltier effects will be reversed, and the regions which in the former case were hot will now be cold, and *vice versâ*. The distribution of resistance will be changed in a corresponding manner, and the equipotential line will be rotated in the opposite direction.

The peculiar thermoelectric properties of copper and iron, discovered by Sir William Thomson, are thus seen to be sufficient to account for Hall's phenomenon in the case of those metals. If the explanation which I have offered be the

correct one, it is clear that Hall's "positive" metals, cobalt and zinc, should, when tested by Thomson's method, exhibit the same thermoelectric effects as iron; that his "negative" metals should behave thermoelectrically in the same manner as copper; and that lead might be expected to be thermoelectrically unaffected by strain. It became exceedingly interesting to ascertain if this was the case, and I therefore proceeded to repeat Thomson's experiment upon all the metals mentioned by Hall. In most cases at least two or three different specimens of the metal were used, and, except in the case of cobalt, the following simple method of operating was adopted.

A short piece of the wire or strip of the foil to be examined was held at the ends by two fixed brass clamps, about 5 or 6 centim. apart, which were connected with the terminals of a low resistance reflecting-galvanometer. A point near the middle of the metal was then gripped by a pair of pliers which had been previously heated, and one half of the wire was drawn taut, leaving the other half slack. A galvanometer-deflection immediately occurred, which ceased when the tension was discontinued. The process was then repeated, the pull being made in the opposite direction: this resulted in an opposite deflection of the galvanometer.

Upon first gripping the metals, especially the harder ones, it frequently happens that before beginning to pull a thermoelectric current is set up between different parts owing to local differences in physical condition, and these thermocurrents are sometimes so strong as to completely mask the effect produced by stretching. Their effect may be practically eliminated by giving a series of short pulls at regular intervals, corresponding with the periodic swing of the galvanometer. The results can thus be observed with perfect accuracy, but the method is not well suited for use before an audience on account of the difficulty of rendering evident the correspondence between the pulls and the consequent galvanometer-deflections. The hand and eye should, in fact, work together*.

* The experiment was, however, successfully exhibited at the meeting. All Hall's metals were used except cobalt, which could not be obtained in the form of wire, and the only doubtful case was that of tin, the wire

The directions of the currents through the hot junctions are given in the following table, which is taken from my former paper, with the addition of the results for wires of tin and lead:—

S = Stretched. U = Unstretched.

Metals.	Forms used.	Direction of current.	Hall's effect.
Copper	Wire and foil; pure	S to U	–
Iron	Wire and sheet; annealed	U to S	+
Brass	Wire, commercial	S to U	–
Zinc	Wire and foil	U to S	+
Nickel	Wire	S to U	–
Platinum	Wire and foil	S to U	–
Gold	Foil, purity 99·9 per cent.	S to U	–
	Wire, commercially pure	*U to S*	
	Jewellers' 18 ct. wire and sheet	S to U	
	Jewellers' 15 ct. sheet	S to U	
Silver	Wire and foil	S to U	–
Aluminium	*Wire and foil, pure*	*U to S*	– ?
Cobalt	Rod; 8 mm. diameter	U to S	+
Magnesium	Ribbon	S to U	–
Tin	Foil and wire	S to U	–
Lead	Foil (assay) and wire	No current	Nil.

It will be seen that, in every case excepting that of aluminium and one out of five specimens of gold, there is perfect correspondence between the direction of the thermoelectric current and the sign of Hall's effect. With regard to the aluminium, a piece of the foil was mounted upon glass, and Hall's experiment performed with it. As was anticipated, the sign of the "rotational coefficient" was found to be + like that of iron, zinc, and cobalt. It is probable, therefore, that the specimen of aluminium with which Mr. Hall worked differed in some respect from that used by myself. The anomalous specimen of gold being in the form of wire, could not be submitted to the same test: it probably contains some disturbing impurity.

With regard to cobalt, since it could not be obtained in the

being ruptured before any marked deflection was produced. It should be noticed that the temperature of the hot junction should not exceed about 100° C., otherwise, at least in the case of iron and nickel, different results may be obtained.

form either of wire or of sheet, it was necessary for the observation of the thermoelectric effect to treat it in a somewhat different manner from that previously described. A rod of the metal was held firmly by one end in a vice, and both ends were connected by wires with the terminals of the galvanometer. The middle of the rod was gripped with gas-tongs which had been made hot in a Bunsen flame, and stress was produced by twisting. It was found that, while the stress continued, a current passed from the strained to the free portion of the rod through the hot part. The direction of the current was the same whether the twist was clockwise or counterclockwise, and it ceased the moment the stress was discontinued. For the sake of comparison, a copper rod was treated in a similar manner, and the resulting current was in the reverse direction. For the purpose of this experiment, torsion is clearly equivalent to traction, both tending to draw asunder the molecules of the metal.

Some months ago, when experimenting with pieces of iron cut into different shapes, I tried the effect of making a longitudinal slit along the middle of the plate. The result was, that the Hall effect was considerably weakened even though a stronger current was used than before the slit was cut. In the light of subsequently acquired knowledge, I have varied the experiment thus :—A plate of iron was prepared as usual, but the galvanometer-wires, instead of being attached in the ordinary manner, were connected to points nearer to the middle of the plate than to the edges. When the magnet was excited, the ordinary Hall effect was found to occur, but it was somewhat weakened. Two longitudinal slits were then cut along the middle of the plate, a connecting piece about 4 millim. wide being left at the centre. A current from two bichromate cells was passed through the plate, and the positions of the galvanometer connexions were carefully adjusted, until, on depressing the key K (see fig. 5), not the smallest movement of the spot of light occurred. The electromagnet was then excited, the key was depressed, and the spot of light was deflected. The magnet-current was reversed, the key was again depressed, and there was again a deflection of the spot of light. And the directions of the deflections were in both cases *opposite* to those which occurred before the slits

were cut in the plate. The amount of deflection thus produced was about 35 scale-divisions *.

Now it is quite clear that, if Hall's effect were due to the direct action of the magnet upon the current, it ought to occur in each of the two halves of the divided plate. The transverse current in the lower half would (supposing its direction to be upwards in the figure) pass from the point B to the metallic bridge at the centre, and the transverse current in the upper half would pass from the centre to the point A. Upon no conceivable hypothesis could the mere division of the plate have the effect of actually reversing any direct action which the magnet might exert upon the current.

The strain theory, however, explains the matter perfectly. Supposing a south pole to be beneath the plate, and the current through it to be flowing from left to right, then, before the cuts were made, the point A would be in a stretched district, and the point B in a compressed district (see fig. 3); and the resulting Peltier effects would cause the transverse current to flow in the direction A G B, fig. 5. But in consequence of the longitudinal cuts, the distribution of stress is altered. Instead of the whole plate, each half of it may now be mapped out into six areas which are differently strained, and the point A will now be in a compressed district, and B in a stretched district. The Peltier effects will therefore be reversed, and the transverse current will flow in the direction B G A. The existence of the central bridge does not prevent the new strain, because there is no shearing at points midway

* The experiment as described is an exceedingly troublesome one, and no attempt was made to exhibit it at the meeting. The difficulty arises from the fact that, in order to show the effect with certainty, a very perfect balance must be obtained, and this alone generally occupies a great deal of time. But even when two exactly equipotential points have been found, it often happens that the mere reversal of the polarity of the magnet disturbs the adjustment of the galvanometer-contacts to such an extent that the effect cannot be observed: more time must therefore be consumed in readjusting the contacts before a successful experiment can be made. Believing this inconvenience to be due to small displacements resulting from the attractive action of the magnet upon the iron, I have since tried the experiment with gold-foil. This turns out to be a much more suitable metal for the purpose, rendering it possible to observe the effect with ease and certainty.

between the ends of the plate; and as regards transverse distortion, the central bridge merely forms a link between the upper and lower portions of the plate, which link shares their movement in the upward direction without impeding it. Thus the reversal of the Hall effect is easily and completely accounted for.

The 'Philosophical Magazine' for last January contains a translation of a paper by Prof. Righi, on Hall's Phenomenon. He uses three electrodes instead of four. The current enters or leaves by one, and leaves or enters by the other two. The two partial currents pass through a "Wiedemann's" (presumably a differential) galvanometer, and zero is obtained by the use of resistance-coils. On exciting the electromagnet a galvanometer-deflection occurs, indicating a current in the opposite direction to that named by Hall.

This also can be explained by strain and Peltier action. Let us suppose, as before, that the plate is of iron, and that the south pole is beneath it. Let a current enter the plate at A, fig. 6, and leave it at the two points D and E by the wires leading to the differential galvanometer G. The plate is divided into six strained regions exactly as in Hall's arrangement, those which are compressed being marked C, and those which are stretched S. But the neighbourhood of the upper electrode D will, in this case, be *heated*, because the current is passing from stretched to compressed: the resistance of the region will therefore be increased. Similarly the resistance of the metal in the neighbourhood of the lower electrode F will be diminished. Thus a greater quantity of electricity will pass by E than by D, and the galvanometer-deflection will be in the opposite direction to that which would have occurred if the connexions had been made in the middle of the plate, in the usual manner. Righi's experiment therefore confirms my explanation of the Hall effect.

The theory which I have ventured to submit seems to me capable of bearing any test which could reasonably be applied to it. Not only does it completely account for the phenomena described by Hall, but it explains anomalies and enables one to predict results. The chief point of difficulty lies in the strain. In conversation on the subject, I have found that, while it is readily admitted that a strain such as is described

might well be produced in a piece of metal a millimetre in thickness, it is by no means so easy to conceive the possibility of a similar strain in a sheet the thickness of which does not exceed a hundredth or a thousandth of a millimetre. But let it be imagined that the thick piece of metal is divided into a hundred or a thousand independent layers, each of which is acted upon by a force equal to a hundredth or a thousandth part of that which acted upon the whole. Then it becomes evident that each individual stratum, including the bottom one, will be affected in precisely the same manner as the sum of them which constitutes the thick plate. Of course, if the film of metal is so exceedingly thin that its physical properties of cohesion and elasticity are impaired, we should expect the effects of extension and compression to be diminished. This is, in fact, the case with thin gold-leaf, not so much on account of its mere thinness as of the inequalities and irregularities necessarily existing in it; and accordingly we find it noted by Mr. Hall (Phil. Mag. November 1880, p. 312) that the transverse effect in gold-leaf appears to be *relatively* much smaller than the effect in strips of sensible thickness*. *Cæteris paribus*, the galvanometer-deflection is, of course, greater with a thin sheet than with a thick one, because the difference of potential at any two points in a thick sheet must always be comparatively small.

If it is objected that, since the plate is firmly cemented to glass, the distortion actually produced must be very small, it may be replied that the consequent effect is also very small, and that a distortion far too insignificant to be visible to the eye would be sufficient to produce it. The warmth communicated to the plate by lightly touching opposite corners of it with the fingers is capable of causing galvanometer-deflections many times as great as those produced by Peltier effects requiring three or four amperes of current for their production.

Lastly, it may be urged that the force available for producing the required strain is a very small one. This is

* Mr. Hall thinks that the apparent difference may be accounted for by the existence of a multitude of small holes in the thin leaf, by impurities in the gold, and by the difficulty of securing good contact with the electrodes.

undoubtedly true. But, apart from the fact that a very small force would be sufficient to cause a sensible strain in a very thin sheet of metal, it must be remembered that the force with which we are dealing acts in an exceedingly advantageous manner. It does not, like an external push or pull, act upon a single point or group of points in the sheet, but it exerts a perfectly independent action upon every individual molecule of the metal which is within the magnetic field, most powerfully in the middle, gradually falling off towards the ends. It acts, in short, in the most effective manner possible for the production of the strain.

Though it may not impossibly turn out, on further investigation, that the details of the action differ in some respects from those which I have suggested, I think no reasonable doubt can remain that Hall's phenomenon is simply a consequence of mechanical action combined with Peltier effects. And if it should be thought that I have, in this paper, brought forward a superfluity of argument in support of a proposition which, when once stated, appears almost self-evident, I can only say that, under the circumstances, and having regard to the great importance of the subject, it seemed desirable that it should receive the most thorough and exhaustive treatment possible.

III. *On the Adjustment of Resistance-Coils.* *By* Professor SILVANUS P. THOMPSON, *B.A., D.Sc., Univ. Coll. Bristol**.

1. ALTHOUGH the existing methods for the exact measurement of the electric resistance of a wire are simple, rapid, and reliable to a very high degree, there is not, so far as I am aware, any simple, rapid, and reliable method yet known of adjusting resistance-coils to their exact value, when some standard coil of known resistance is used for comparison. It is, of course, much more important to know what the exact value of a coil is than to have that coil adjusted to one ohm or one hundred ohms, if such adjustment involves some uncertainty in the percentage of its precise value. But I do not see why our standards should not only be accurately

* Read February 23, 1884.

known, but also accurately adjusted to standard values, at least with far greater precision than is often the case. Further than this, the great cost of reliable resistance-coils, which hinders in many cases their being purchased by individuals and institutions of limited means, arises almost entirely from the high cost of the skilled labour now needed for the tedious processes of adjusting the coils by trial and error to something approaching their nominal values.

The method of adjusting resistance-coils now suggested has proved itself in practice far more accurate than any other, and the rapidity with which adjustment can be secured is extraordinary. It consists in taking as a first approximation a wire cut off roughly of such a length as to have a resistance slightly greater than that to be finally given, and then shunting the whole or a portion of this coil by a coil of much higher resistance, also approximately cut off to a resistance calculated from a single careful measurement of the first rough coil. The method, though new for the construction and adjustment of permanent coils, is not new as a method in general, as it was used nearly twenty years ago by the Committee on Electrical Standards for temporary adjustments. On page 101 of the Reports, as reprinted under the editorship of Prof. Fleeming Jenkin, the following passage occurs :—

"The idea of using large coils combined with small ones in multiple arc to obtain extremely small minute differences of resistance was suggested to the writer by Professor [now Sir] W. Thomson, and will be found useful in very many ways." But only one way is mentioned in the Reports, namely that of plugging in the high resistance as a temporary shunt during a single experiment.

2. I will first describe the method pursued in the Physical Laboratory of University College, Bristol, for the adjustment of coils not exceeding 10 ohms in resistance. First, the length, calculated to within 0·5 of a centimetre, is cut off from a specimen of German-silver wire of which the resistance per centimetre is roughly known; 2 per cent. being added before the wire is cut off. It is soldered to the stout copper terminals prepared to receive it, doubled on itself, twisted, wound up, and when the soldering has cooled, the resistance of the wire

is measured with the utmost care by comparison, made on Professor Carey Foster's method, with a standard coil, constructed at Cambridge in Professor Stuart's workshops, and which has been verified at the Cavendish Laboratory and certified in terms of Lord Rayleigh's determination of the ohm. The "rough resistance" of the measured coil being thus very accurately known, a calculation is made to ascertain what resistance, connected as a shunt, will reduce the resistance between terminals to one exact Rayleigh ohm. The resistance required for the shunt is in practice from 12 to 50, 60, or even 80 ohms. The nearer the first approximation, the higher the shunt-resistance required, as the formula given below shows at a glance. The calculation then having been made, the requisite length to approximate to this is cut off from a much finer wire, which is then soldered across the terminals. At first we used to measure the resistance of these shunts before putting them on. In practice we find this quite unnecessary, as they can always, if there is any error of importance, be readjusted after soldering them to the copper terminals. In nine cases out of ten, however, no further adjustment is needed.

3. The following is the theory of, and formula for, the value of the shunt required.

Let $\mathfrak{R}$ be the exact resistance of the first rough approximately-cut coil, being larger by from 1 to 4 per cent. than the final value. It is required to find the value S of a shunt-coil which will reduce its resistance to the final exact value R.

By the ordinary rule for divided circuits we have

$$R = \frac{\mathfrak{R}S}{\mathfrak{R}+S},$$

$$R(\mathfrak{R}+S) = \mathfrak{R}S ;$$

whence

$$\frac{\mathfrak{R}R}{\mathfrak{R}-R} = S.$$

4. An example for the adjustment of a 1-ohm coil will illustrate the method and its advantages. The following data are taken from the note-book of Mr. C. C. Starling, Demonstrator in the Physical Laboratory of University College, Bristol, January 30, 1881. Coil marked "ϕ 3."

1·1 metre of German-silver wire (No. 24 B.W.G.), diam. 0·56 millim., cut off and soldered to terminals. Tested (by Foster's method) against Cambridge Unit No. 6.

$$\mathfrak{R}=1{\cdot}0385518 \text{ (temp. } 17^\circ{\cdot}2 \text{ C.)}$$

$$\frac{1{\cdot}0385518}{{\cdot}0385518}=33{\cdot}9 \text{ ohms required as shunt.}$$

7·05 metres of German-silver wire (No. 36 B.W.G.), diam. 0·2 millim. (at 20·8 centim. per ohm), cut off, and without further measurement soldered to terminals. *Immediately* tested by Foster's method,

$$R=1{\cdot}007912 \text{ Rayleigh ohms (temp. } 17^\circ{\cdot}3 \text{ C.).}$$

Time required to make and test coil and shunt 38 *minutes.*

This coil was tested again three days later, namely on February 3, and gave

$$R=1{\cdot}004178 \text{ (temp. } 13^\circ{\cdot}5 \text{ C.).}$$

Tested again, February 5,

$$R=1{\cdot}0050854 \text{ (temp. } 14^\circ{\cdot}5 \text{ C.).}$$

Tested again, February 21,

$$R=1{\cdot}0063923 \text{ (temp. } 15^\circ{\cdot}5 \text{ C.).}$$

This coil is therefore correct at 0°·97 C.

One other example of the application of the method to a bad case will suffice. In this instance the resistance of the main wire was not near enough to the final value, being more than 14 per cent. too great, and the shunt consequently shorter than desirable. Nevertheless the result was quite satisfactory.

One-ohm coil made in Physical Laboratory, University College, Bristol, by Mr. C. Swan,

$$\mathfrak{R}=1{\cdot}14599.$$

Shunt required 7·85 ohms. Cut off and soldered. Final testing by Foster's method, as follows :—

U=0·9890385 *Rayleigh ohms.* (This is the standard "Cambridge Unit" verified at the Cavendish Laboratory.)

$x'=68{\cdot}845$ (mean of three readings).

$x\ =65{\cdot}280$ " " "

$\mu =$ 0·0031 ohm per centimetre of the bridge wire.
$R = U - \mu(x - x')$.
$R = 1{\cdot}0000900$ *Rayleigh ohms* (temp. 16° C.).
[Tested again next day this same coil gave
$R = 1{\cdot}0000435$ *Rayleigh ohms* (temp. 17°·2 C.).]

5. The amount of the final error caused by any given error in the shunt is easily expressed by a formula. Let r be the error of the shunt, and n the ratio of the shunt to the unshunted resistance. In practice $60 > n > 10$. If the shunt S were without error we should have, from the usual formula,

$$R = \frac{\mathfrak{R}S}{\mathfrak{R} + S};$$

or, if S were truly equal to $n\mathfrak{R}$,

$$R = \frac{n\mathfrak{R}}{1 + n}.$$

But in reality $S = n\mathfrak{R} + r$, and consequently the shunted coil has for its slightly erroneous final resistance,

$$R' = \frac{\mathfrak{R}(n\mathfrak{R} + r)}{\mathfrak{R} + n\mathfrak{R} + r}$$
$$= \frac{n\mathfrak{R} + r}{1 + n + \frac{r}{\mathfrak{R}}}.$$

Therefore we get as the final error,

$$\epsilon = R' - R = \frac{r}{(n+1)^2 + \frac{r}{\mathfrak{R}}(n-1)}.$$

The second term in the denominator is always small compared with the first term, and may be omitted, for n is never less than 10, as remarked above, and r is very seldom one fiftieth part of $\mathfrak{R}$, and need not, with care, exceed one two-hundredth. This reduces the expression for the error to

$$\epsilon = \frac{r}{(n+1)^2}.$$

Suppose the error r made in measuring the shunt is as much as 2 per cent., and the value of the shunt-ratio n only 10; then, as the formula shows, the final error is only ·0165 per cent., or in a 1-ohm coil amounts to 0·000165 ohm. But if, as is in practice much more frequently the case, the shunt-

ratio n is 50 instead of 10 (and often more), then with a 2-per-cent. error in the shunt the final error is only ·000767, or in a 1-ohm coil amounts to 0·00000767 ohm. In practice, the error made in cutting off the shunt wire is nothing like 2 per cent.: one half per cent is easily attainable.

6. The special advantages possessed by this method of adjustment cease to be available for resistances exceeding 10 ohms, as it is not convenient to measure off shunts of 500 to 800 ohms; for the wire is either inordinately long, or too thin for a reliable proportion to exist between length and resistance. For resistances exceeding 10 ohms we therefore modify the process of adjustment as follows. The wire is roughly measured out as before, to a value slightly exceeding the final correct resistance. It is soldered to the usual stout copper terminals; but before it is doubled, twisted, and wound up, a spot is bared at about one tenth or less of its length from one end. At this point a short piece of copper, about 2 millim. thick and 3 centim. long, is soldered. The resistance of the whole wire is then very accurately measured by Foster's method, and the resistance of the short portion between the terminal and the copper piece is also very accurately measured. We next calculate the value to which the resistance of this short piece must be reduced in order to bring the total resistance to its correct desired value, and then the short portion is shunted down to the required amount by a shunt-wire calculated, measured out, and soldered on, exactly as described before.

7. In conclusion, I would remark that in advancing from coils of 1-ohm to coils of higher values we have followed the suggestion made by Lord Rayleigh in 1881, of passing by three threes plus one to ten, and three thirties plus ten to one hundred. We have, however, found it much more convenient not to use the ingenious but complicated system of mercury-cups suggested by Lord Rayleigh for facilitating this operation. Instead of this we have employed the following simpler device. Six holes are drilled in a block of paraffin for mercury-cups. The holes have sheet-copper disks fixed at the bottom. Each cup is large enough to admit of the insertion of three stout copper terminals. Figures 1 and 2 below show how the

arrangements are made for connecting the three threes in parallel, for verification, and for connecting the three threes plus one in series, when using them for testing a 10-ohm coil. In both figures the letter *c* denotes a connecting bar of stout copper.

Fig. 1.

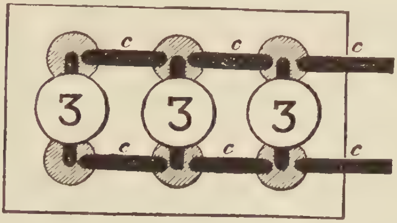
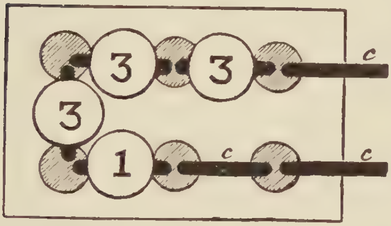


Fig. 2.



My thanks are due to my assistant, Mr. Colman C. Starling, Demonstrator in Physics in University College, Bristol, for having zealously aided me in working out the methods of adjustment described above.

IV. *Note on the Electric Conductivity and other Properties of the Copper-Antimony Alloys. By* George Kamensky, *Assoc. Royal School of Mines**.

It is remarkable that the physical constants of the alloys of copper and antimony have hitherto been but little examined, more especially as the series is a very striking one; and the so-called "Regulus of Venus," which was well known to the alchemists, stands out prominently, with its beautiful violet colouring, both from the rest of the copper-antimony series and from the alloys of other metals.

* Read June 23, 1883.

As a typical set to work upon I selected the following series :—

No.	1.	Antimony	100 parts.		Copper	0 parts.	
„	2.	„	95	„	„	5	„
„	3.	„	90	„	„	10	„
„	4.	„	85	„	„	15	„
„	5.	„	80	„	„	20	„
„	6.	„	75	„	„	25	„
„	7.	„	70	„	„	30	„
„	8.	„	65	„	„	35	„
„	9.	„	60	„	„	40	„
„	10.	„	55	„	„	45	„
„	11.	„	50	„	„	50	„
„	12.	„	45	„	„	55	„
„	13.	„	40	„	„	60	„
„	14.	„	35	„	„	65	„
„	15.	„	30	„	„	70	„
„	16.	„	25	„	„	75	„
„	17.	„	20	„	„	80	„
„	18.	„	15	„	„	85	„
„	19.	„	10	„	„	90	„
„	20.	„	5	„	„	95	„

I afterwards found it necessary to prepare two more, viz.:— No. 10*a*, Antimony 49, Copper 51, which has the formula $Cu_2 Sb$; and No. 13*a*, Antimony 31·9, Copper 68·1, which possesses the formula $Cu_4 Sb$.

From the point of view of the metallurgist, the most interesting fact connected with the electrical constants of the alloys is the determination of the group to which, following Matthiesen's classifications*, they belong. Are the alloys of copper and antimony like, for instance, those of copper and tin, "solidified solutions of one metal in the allotropic modification of the other"; and if so, is the curve of their electrical resistance a *continuous* one, showing at first a rapid fall on the copper side, and then turning and passing nearly horizontal to the point representing unalloyed antimony?

The entire series of the copper-antimony alloys is unfortunately very brittle, and therefore the preparation of wires is out of the question, and the determination of the electrical resistance of cast rods of metal is known to be attended with many difficulties. I therefore determined to follow the course

* Phil. Trans. British Association Reports, 1863.

indicated by Prof. W. Chandler Roberts*, who has shown that the necessary information as to the electrical position of alloys may be readily ascertained by the induction-balance; and the accuracy of his view was subsequently confirmed by Dr. Oliver Lodge†, who used the Wheatstone bridge. The method adopted in the case of the induction-balance consists in placing the disk to be examined on one side of the balance, and in superposing a graduated wedge-shaped scale of zinc over the opposing coil. This method has already been described by Prof. Roberts‡, who has lent me the graduated zinc scale by which his numbers representing the "induction-balance effect" of various alloys were made.

It is true that the numbers named by the use of this scale are arbitrary; but they can be controlled by employing disks of pure metal of known resistance, provided that they have the same dimensions and volume as the disks of the alloys under examination.

The alloys were cast direct into the required disks, which was managed by using an ingot-mould in which a steel plate cut out, as here shown, was inserted. The disks so cast were ground down on a stone to a thickness of 3 centim.

In mapping out the curve of Electric Conductivity, I decided, after consultation with Prof. C. Roberts, not to give the number obtained for the disk of copper, as in his opinion that cannot be determined for a disk of the same size as the rest of the series by the particular zinc scale used.

I here give the readings obtained with the balance:—

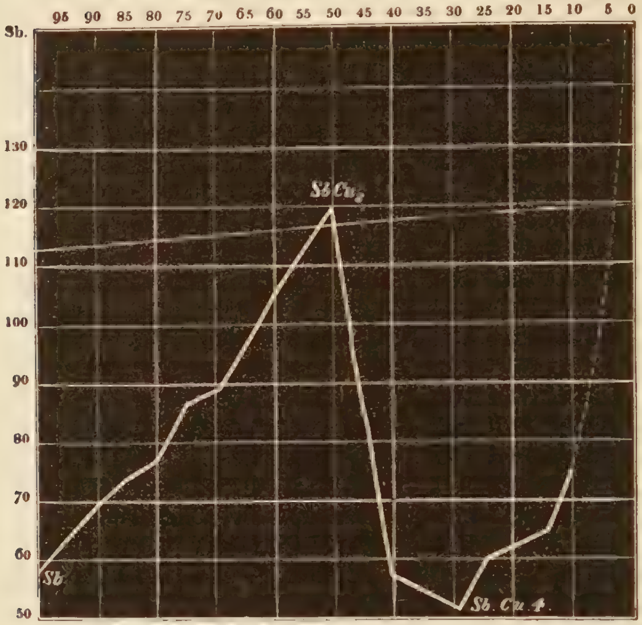
No. 1.	58	No. 11.	85
„ 2.	69	„ 12.	57
„ 3.	74·5	„ 13.	52
„ 4.	77	„ 13*a*.	48
„ 5.	87	„ 14.	53
„ 6.	89	„ 15.	60
„ 7.		„ 16.	63
„ 8.	105·5	„ 17.	65
„ 9.	120	„ 18.	77
„ 10*a*.	121		

* Phil. Mag. July 1879.

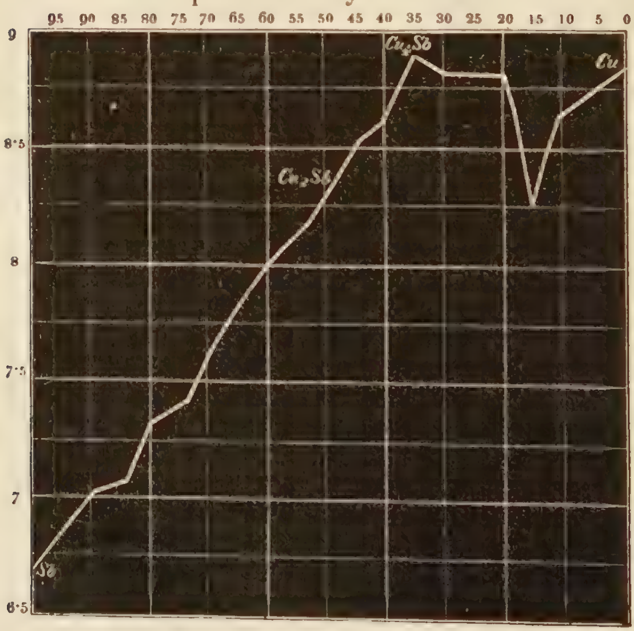
† Phil. Mag. February 1880.

‡ *Loc. cit.*

Curve of Electric Conductivity.



Specific-Gravity Curve.



It will be seen that the curve, viewed as a whole, is of the ∠ shape, which Matthiesen found to be characteristic of the class of alloys of which the copper-tin series is the type. There is a very rapid fall from copper to the alloy containing only 15 per cent. of antimony; and this decrement is continued until the alloy $SbCu_4$ is reached, when the curve turns rapidly and rises to $SbCu_2$, and then turns again and passes to pure antimony. It is not a little interesting that, in the copper-tin series (Roberts), the alloy $SnCu_4$ occupies the lowest part of the curve, precisely the position that, in the copper-antimony series, is occupied by the alloy $SbCu_4$. In the copper-tin series, however, the second critical point is held by the $SnCu_3$; and this point in the copper-antimony curve is assumed, *not* by $SbCu_3$, but by $SbCu_2$.

I may add that I have confirmed the composition of the alloys at these critical points by analysis.

	$SbCu_2$.		$SbCu_4$.	
	Analysis.	Theory.	Analysis.	Theory.
Copper	50·85	50·5	68·38	68.1
Antimony ...	49·15	49·5	31·62	31·9
	100·00	100·0	100·00	100·0

In determining the specific gravities, I find that they rise gradually and evenly from antimony to the alloy Cu_4Sb (whose specific gravity is 8·871), and then diminish to the specific gravity of copper. The "Regulus of Venus" has a specific gravity 8·339, which does not in any way stand out from the rest.

Sb.	Cu.		Specific gravities.
100	0	....	6·700
95	5	....	
90	10	....	7·005
85	15	....	7·080
80	20	....	7·306
75	25	....	7·407
70	30	....	7·615
65	35	....	
60	40	....	7·995
55	45	....	8·194
50	50	(Cu_2Sb)	8·339
45	55	....	8·504

Sb.	Cu.		Specific gravities.
40	60	....	8·617
35	65	(Cu_4Sb)	8·871
30	70	....	8·823
25	75	....	8·811
20	80	....	8·802
15	85	....	8·262
10	90	....	8·637
5	95	....	8·727
0	100	....	8·85

The following is a description of the fractures of the series:—

No. 1.	....	Bright white, highly crystalline.
„ 2.	....	Dull white, highly crystalline.
„ 3.	....	Grey, highly crystalline.
„ 4.	....	Grey, finely crystalline.
„ 5.	....	Dull fibrous, crystalline.
„ 6.	....	Bright purplish, highly crystalline, more than No. 1.
„ 7.	Very brittle.	Bright purplish, large plates of crystal.
„ 8.	....	Bright purplish, smaller plates of crystal.
„ 9.	....	Bluish, very finely cryptoconchoidal.
„ 10.	....	Purple, fibrous.
„ 11.	....	Purplish grey, fibrous.
„ 12.	....	Bluish grey, conchoidal.
„ 13.	....	White vitreous, fine granular.
„ 14.	....	Yellowish white, fine granular.
„ 15.	....	White, extremely fine granular.
„ 16.	....	Grey, granular.
„ 17.	....	Reddish grey, granular.
„ 18.	....	Orange, coarse granular.
„ 19.	....	Light red, granular.

The alloys from Nos. 12 to 16 are easily tarnished.

From this it will be seen that antimony is very persistent in its influence, and also that the colour of the "Regulus of Venus" first appears in the alloy containing 75 per cent. of antimony, and disappears in that containing 45 per cent.

From these data it would appear that in the copper-antimony series there are two alloys which may be assumed to possess very definite compositions—viz. the "Regulus of Venus," $SbCu_2$, and the alloy represented by the formula $SbCu_4$.

V. *Direct-Reading Electro-Measuring Instruments, and a Non-Sparking Key.* *By* Professors W. E. Ayrton, *F.R.S., and* John Perry, *M.E.**

[Plate III.]

The construction and use of our various measuring-instruments are now so sufficiently well known that it is unnecessary for us to give any general description of them; indeed the names "Ammeter," "Voltmeter," by which we ventured to christen two of our children, have found favour in the eyes of Electricians, and are now current in electrical literature as the typical names, or surnames, of instruments used to measure strong currents and large electromotive forces respectively. But just as improvements in electrical measurements have led, step by step, to the names for the electrical units receiving more and more exact definitions, so we now propose to take advantage of certain important improvements that we have recently effected in our measuring-instruments to define the names "Ammeter," "Voltmeter," and "Ohmmeter" more exactly.

Proportional Law.—In the earliest form of instruments commercially employed for measuring the large currents used for electric lighting, viz. the instruments of M. Deprez, reference had to be made to a table of values to ascertain the meaning of any particular deflection, since there was no immediate connexion between 1 degree deflection and 1 ampere; and, further, it was necessary to refer to the table twice over when measuring two different currents, as the deflections were not proportional to the current. By giving a proper shape, however, to the coil, needle, and pole-pieces of the controlling permanent magnet, we succeeded (in 1880) in producing instruments in which the current or potential difference was exactly proportional to the deflection throughout the whole range of the scale; and, further, by the employment of our commutator principle an instrument was obtained having two degrees of sensibility, one exactly ten times the other; so that a commutator ammeter, or voltmeter, although employed to measure strong electric currents and large potential differences respectively, could be calibrated by the

* Read January 26, 1884.

employment of a single cell of known electromotive force but of totally unknown resistance.

Objections to the Employment of a Constant.—But these instruments possessed one serious fault looked at from the user's point of view, and two others which especially concerned the manufacturer. The scales of all the instruments were divided into degrees; and although the deflections were proportional respectively to the current or the potential difference to be measured, still there was nothing to show to how many amperes or volts one degree of the scale corresponded. Consequently for each instrument there was a "Calibration Constant," such as 1·75 ampere or 2·14 volts per degree, which was determined experimentally by the makers. We have here two of such old type of voltmeter, in one of which 5·7 volts produce one degree deflection, and in the other 0·09 volt produces one degree; the former being intended for measuring electromotive forces up to about 250 volts, the other the electromotive force of a single cell. But the employment of such a constant had the very serious objection that, as all the instruments looked exactly alike, there was the great danger in any factory, where several of these instruments were in use, of the constant for one instrument being used accidentally for that of another. And this mistake not only renders the answer, of course, quite wrong, but is liable to lead to the destruction of the instrument, or at any rate to the breaking of the pointer, from, say, an instrument having a constant 0·12 ampere per degree being put on a circuit through which 50 amperes are flowing, in consequence of the experimenter mistaking the "constant" for 1·2 ampere per degree. Further, even if no mistake be made in the "constant," it is troublesome, except for those who are expert in mental calculation (a quality not always predominating in the men in the charge of electric-light circuits), to multiply a "constant" 3·17 by the deflection, say 21·32, quickly without the employment of paper and pencil.

We therefore decided, not merely to avoid using tables of values such as had to be employed by M. Deprez, but even to abandon the use of constants altogether, and to arrange matters so that the pointer pointed at once throughout the whole range of the scale to the number of amperes or volts to be

measured ; to design, in fact, "Direct-Reading" instruments. And for the future we propose to confine the definition of an "Ammeter," a "Voltmeter," and an "Ohmmeter" to instruments on which respectively amperes, volts, and ohms can be immediately read off without any calculation or any reference to a constant or table of values.

On the table there are various specimens of our new Direct-Reading Ammeters and Voltmeters. In each case the dial is a complete disk (see Plate III. fig. 1), and not a sector as in the old instrument. The light aluminium pointer is also much longer, and the length of the graduated arc over which its end moves is much greater, so that readings with the new instruments can be made much more accurately than in the old. Here is a direct-reading ammeter suitable for measuring up to 10 amperes, correct to a tenth of an ampere ; here another for measuring up to 150 amperes, each division of the scale corresponding with one ampere. I now send a current with these accumulators through this thick German-silver wire, and you see at once from the deflection on this ammeter, without any reference to a table of values, and without the employment of any troublesome constant, that the current passing, and which is now making the wire white-hot, is 207 amperes. Again, each division on the scale of this voltmeter corresponds with 1 volt, this particular instrument being suitable for measurements up to 100 volts.

To construct a direct-reading instrument it is necessary to employ some form of adjustment for sensibility, since to have a special scale engraved for each instrument after it was wound would be both troublesome and expensive ; and, further, if there were no power of adjustment, any change in the strength of the permanent controlling magnet would prevent the instrument from any longer giving one division-deflection per ampere or per volt—in other words, would necessitate the employment of a constant.

Difficulties of Manufacture of Older Forms.—As regards the two defects from the manufacturer's point of view they were these:—Even although the shape of the coil, needle, and pole-pieces of the magnets were made accurately to templates, it was found difficult, when very thick wire had to be coiled on the bobbin of an instrument for measuring strong currents, to

wind it with certainty so that both the deflection was proportional to the current, and that equal deflections on the two sides of the zero had exactly the same value. As long as the wire was thin, symmetry of winding was comparatively easy to obtain; but when thick wires, having but a few convolutions, were wound round a small bobbin, the thickness of the wire, combined with the galvanometric importance of each of the few convolutions, especially of those closest to the needle, made it necessary to wind and rewind some of the instruments several times before the proportional law, as well as equal deflections right and left, could be obtained.

Adjustment of Direct-Reading Instruments.—It therefore was important, to avoid complexity of construction, that the adjustment for sensibility referred to above should, if possible, be so contrived as to serve also for an adjustment for the proportional law. And this we are happy to say we have at last succeeded in accomplishing, by fitting into each end of the bobbin round which the wire is wound specially constructed charcoal-iron cores fitted with a fine screw. If these cores be too far out or out altogether, the proportional law will probably not be true, the current increasing more rapidly than the deflection; on the other hand, if they be screwed fully home the proportional law will also not be true, the deflection now increasing more rapidly than the current; but we find that between two limiting positions of these iron cores the proportional law is true; and by moving the cores in or out within these limits, the sensibility of the instrument can be much altered without destroying the proportional law.

To construct then, say, a 50-ampere instrument, wire is selected of such thickness, first, that 50 amperes will not heat it too much; secondly, that a sufficient number of convolutions can be wound on the bobbin for 50 amperes to deflect the needle to the limit of the scale when it is controlled by a permanent magnet of normal power. A scale divided for 50 amperes (that is, having the division marked 50 amperes at the end of its range) is then put on, and the soft-iron cores screwed in or out a little by trial until the pointer is found to point to exactly the same number on the dial as the number of amperes of current flowing through the coil. If at any time it be found that the permanent magnet has lost some of

its magnetism, so that less than, say, 40 amperes will deflect the pointer to 40 amperes on the dial, the iron cores are unscrewed out a very small distance until the change in the magnetism of the controlling magnet is exactly compensated for.

The other adjustment to be employed by the makers for equalizing the deflections right and left for the same current is effected by fixing the coil so that it can turn about its centre.

The "Ten Law" of our commutator-instruments, which means that an ammeter is exactly ten times as sensitive when the commutator is turned to series as when to parallel, while a voltmeter is ten times as sensitive when the commutator is turned to parallel as when turned to series, is effected by the ten wires forming the bobbin having, inclusive of the resistance of the contacts at their ends, exactly the same resistance; which condition being fulfilled, it is quite unnecessary for obtaining the ten-law that the ten wires composing the strand should be at all symmetrical in the way in which they are wound on the bobbin. With some of our earlier commutator-instruments the resistance of the contact of the springs with the metallic pieces of the commutator introduced an error when the ten wires were each thick and short, since then the terminal resistance was a very material portion of the whole resistance of any one of the ten wires. To avoid this the commutators and the springs, as will be seen from the various specimens of the instruments on the table, which, Darwinian-like, show the history of the survival of the fittest, have assumed a much larger form.

In our direct-reading commutator-instruments the scale is double, as may be seen from fig. 2; the upper numbers corresponding with the number of volts when the commutator is to parallel, the lower when to series; while in a Direct-Reading Commutator Ammeter the upper numbers correspond with the number of amperes when the commutator is to series, and the lower when to parallel.

Fig. 3 shows the interior of the latest form. A A is the bobbin round which the wire is coiled; M M is the permanent controlling magnet, made in two parts to obtain considerable strength; P P are the soft-iron pole-pieces of

special shape, to be referred to further on ; F F are the soft-iron cores, and B B is a connecting bridge-piece of charcoal-iron used in our latest form of instrument for increasing the strength of the deflecting field for a given current by connecting magnetically these iron cores together. This addition is very important in voltmeters ; since, although in an ammeter the constant of the instrument is nearly independent of temperature, this is not the case in a voltmeter. The current producing a given deflection in either instrument is practically independent of temperature ; but the potential difference necessary to be maintained at the terminals of the instrument to produce the current, and which potential difference is what the voltmeter is designed to measure, depends necessarily on the resistance and therefore on the temperature of the coil. A temperature-correction can be easily made for the portion of the effect arising from the temperature of the room ; but the unknown amount of heating of the coil arising from the passage of the current is far more difficult to allow for. Hence it is generally necessary to be contented with a weaker controlling permanent magnet for a voltmeter in order that the magnetic effect necessary to produce any given deflection, and consequently the heating effect which is proportional to the square of this magnetic effect, shall be less in the case of a voltmeter than in an ammeter. But the employment of our soft-iron pole-pieces combined with the soft-iron bridge enables us to use much more powerful controlling magnets, and therefore to obtain a much greater dead-beatness, and a greater freedom from disturbance produced by extraneous powerful magnets with our last form of voltmeters than with our earliest.

German-Silver or Copper Wire for the Coil of a Voltmeter.— It might appear at first sight that German-silver wire would be better than copper wire for voltmeters. As regards the error arising from the change of temperature of the room this is undoubtedly the case, but not as regards the heating of the coils by the passage of the current. For, to produce any given deflection on a voltmeter provided with a given controlling permanent magnet requires a given current for a given number of convolutions ; hence, since the specific resistance of German silver is about 13 times that of copper, the production of heat, which necessarily accompanies the production of

a given magnetic effect with a given instrument, will be 13 times as great if German silver be used than if copper be used. And on account of the inferior heat-conducting power of the German silver the elevation of temperature will be probably more than 13 times as great. But for the same elevation of temperature the increase of resistance of copper is only 8·8 times the increase of resistance of German silver. Consequently copper wire will be decidedly better than German-silver in any case where the change of resistance due to the heating produced by a current flowing through a given coil is concerned.

The question whether all the wire should be wound on the bobbin of the voltmeter, or whether a portion only should be on the bobbin, and the remainder of the resistance, necessary to be employed to prevent the voltmeter shunting too much of the main current, should be in the form of a resistance-coil, must be answered by the consideration as to whether the total consumption of energy which takes place when a particular deflection is maintained in the voltmeter, or the variation, arising from the heating of the bobbin, of the reading for a fixed potential difference, is the more important.

If it is desired that a particular deflection of the needle shall be maintained with the *least* total consumption of energy, then all the wire should be wound on the bobbin of the voltmeter; whereas if the heating-error is to be reduced to a minimum, then a large portion of the total resistance in the voltmeter circuit should be external to the voltmeter itself, and should be in the form of a resistance-coil made of German-silver wire, of sufficient thickness that its resistance is not sensibly increased when the current necessary to deflect the pointer of the voltmeter to the limit of its scale is maintained.

Methods of Construction for obtaining the Proportional Law. —Next, with reference to the principle that must be followed to obtain the proportional law in galvanometers. In an ordinary galvanometer controlled by the earth-magnetism, the strength of the controlling field is a uniform one; whereas the strength of the deflecting field produced by the current flowing round the coil is strongest in the plane of the coil, and grows weaker as we recede from that plane. Hence, when the needle is deflected, it moves into a weaker part of the deflecting field. The result of this is that the deflections for

different currents increase much less rapidly than the current. If both fields be uniform for all positions of the needle, and be at right angles to one another when the needle is in its zero position, then, as is well known, the tangent of the deflection increases proportionately to the current. In order, then, that the deflection may increase proportionately with the current, one of three conditions must be fulfilled:—(1) the needle when deflected must move into a stronger part of the deflecting-field, or (2) must move into a weaker part of the controlling-field, or (3) both these results must be true. The first condition (uniformity of controlling-field combined with increase of strength of deflecting-field) can be fulfilled by using two solenoids at a small distance apart from one another, but with a common axis, and suspending the needle between them so that, on being deflected, it enters the solenoids and therefore moves into a stronger part of the solenoid field; or it may be fulfilled by a plan carried out practically by two of our assistants, Messrs. Walmsley and Mather, and which consists in suspending a magnet over two coils, C C, wound as shown by the dotted lines in fig. 4, the distance between the coils being rather less than the length of the needle.

This latter mode has been employed in our laboratory for making students' rough galvanometers, a specimen of which you see in its embryo state on the table, and with which the deflections are accurately proportional to the current at any rate up to 45° or 50°. If, however, a dead-beat instrument is required, then, in order to ensure roughly uniformity of controlling-field, the pole-pieces, P P, of the permanent magnet, M M, must be hollowed out, as shown in fig. 5, and must embrace the tube, T, in which the needle moves. We have made a number of ammeters and voltmeters of this type, and a sample one dissected is on the table. The augmentation of strength of the deflecting-field is still further produced by the employment of the soft-iron cores F F referred to above, and which screw into the brass bobbin A A.

But the condition 3 (the needle moving into a stronger part of the deflecting-field and also into a weaker part of the controlling-field) is the one we find the best, and it is the one we are using in all our latest experiments, the interior of one of which is shown in fig. 3.

Change of Strength of the Controlling-Magnet.—In some of our instruments we have found that the constant did not suffer sensible change during a period of many months; in others that the change during a much less time, arising from the falling-off of the strength of the controlling-magnet, was very serious. This latter effect we believe to be partly due to inferiority in the steel, it being very difficult to obtain successive deliveries of magnet-steel from the same manufacturer equally good in quality; and partly from the way in which the armature, attached to the instrument when not in use, was removed from the magnet when about to be used. In some of our older instruments, from the form of construction, the mode of removal of the armature took rather the form of a pull, which is known to be bad for the permanency of a magnet. Consequently our new Non-Commutator Instruments are so arranged as to make it impossible to use armatures at all, while in our Commutator Instruments armatures are attached; but the instrument is so constructed that the armature A (fig. 2) can only be removed by a pure sliding motion; and we shall thus be able to ascertain experimentally whether no armature at all, or an armature put on and removed by a pure sliding action, is the better for keeping the controlling-magnet constant in its strength. We anticipate, however, that no armature at all will be the preferable plan.

Ohmmeters.

An ohmmeter, as we have already said, we define to be an instrument in which the pointer points at once to the number of ohms required to be measured. The method we employ for making such an instrument consists in fixing two coils, at right angles to one another, acting on the same soft-iron needle. One of these coils, having terminals T T (Pl. III. fig. 6), is made of thick wire, and is placed in series with the resistance to be measured; while the other, having terminals *t t*, is composed of very fine wire, and is put as a shunt to the unknown resistance. Hence the main current produces its effect by means of the thick wire coil, and the difference of potentials at the terminals of the unknown resistance on the fine wire coil. Now by properly proportioning the shapes of coils, and by winding these coils in a definite way, we have

obtained an instrument in which the deflection of the pointer is exactly proportional to the number of ohms, and which we find most convenient for measuring the resistance of any parts of an electric-light circuit; such as the field-magnet of a dynamo, or the resistance of an arc-lamp while the strong current is flowing. The thick coil is always kept, like an ammeter, in the main circuit, and the terminals *t t* of the fine wire coil are attached to any two points, the resistance between which, at any particular moment, it is desired to know. The use of this instrument also permits the employment of an iron wire, or even of a bit of wet rope, as a resistance-coil for experimental purposes, the resistance of the iron wire or of the wet rope being determined with the ohmmeter at the moment the experiment is being made.

Sir William Thomson has recently proposed a new unit, a "Mho," the reciprocal of an ohm, on account of the difficulty which is found in making people realize that one thousandth of an ohm may be of great importance when you are dealing with a large current. But we do not propose graduating our ohmmeters as mhometers, an operation of course quite simple to effect; because, first, if it be found that practical men cannot be got to pay attention to a thousandth of an ohm, we venture to think that they can probably be got to appreciate its value if it be called a thousand microhms; just as practical men do not disregard the capacity of a mile of submarine cable when called, as it always is, a third of a microfarad, although, possibly, they might do so if it were spoken of as a three millionth of a farad; and, secondly, a still greater objection to the adoption of the word "mho," ingenious as is this suggestion of Sir William Thomson's (like all his other suggestions), is that the constants in connexion with conductivity, the reciprocal of resistance, are quite different from those connected with resistance. For example, take the case of increase of resistance with temperature. Let R be the resistance of a wire at a temperature $t°$ C., and R_0 that at 0° C.; then

$$R=R_0(1+at),$$

where a is the increase of resistance per ohm per degree. Now let C and C_0 be the respective conductivities; then

$$C=C_0(1-a't),$$

where a' is the decrease per unit of conductivity per degree of temperature.

But since
$$R=\frac{1}{C},$$
$$\text{and } R_0=\frac{1}{C_0},$$
$$\therefore\ 1+at=\frac{1}{1-a't}.$$

When $a\,t$ and $a't$ are small, as in German silver, the percentage increase of resistance per degree equals the percentage decrement in conductivity per degree; but if $a\,t$ is large, as in copper, this is not the case; so that while ·388 is the percentage increase of resistance of copper per degree, ·293 is the percentage decrement of conductivity per degree.

Non-sparking Key.—Various so-called non-sparking keys have been devised during the last year or two; but, as far as we are aware, they by no means completely fulfil their object. On breaking a circuit through which a currrent is flowing, a spark is produced the brilliancy of which depends on the coefficient of self-induction of the circuit, and on the current flowing through the circuit just before the circuit was broken. To completely avoid this spark, the energy stored up in the circuit must be utilized to send a current through an unbroken circuit, and this can be accomplished if the resistance of the circuit, instead of being suddenly varied from a small value to infinity, is increased steadily by the insertion of a resistance increasing at a proper rate. If it increases too quickly, a spark will certainly take place; if too slowly, the wire of the resistance-coil which has been added to the circuit will be dangerously heated.

Fig. 7 (Plate III.) shows an arrangement which we have devised for introducing this resistance at a proper rate, *independently* of any control on the part of the person stopping the current. To start the current, the handle H is turned moderately quickly until a solid piece of metal comes into contact with the brush B, when a catch holds the key in that position, and which is the position shown in the figure. The current now passes from one of the binding-screws to the other through practically no resistance. To stop the current the little handle, h, is slightly pulled, when a spring, S, which

was coiled up when the handle was turned round to put the current on, causes the cylinder to turn in the opposite direction, the various metallic pins on the right-hand side coming, in succession, into contact with the brush B, until finally the pins M made of ebonite, and not of brass, come into contact with the brush B, and the circuit is entirely broken. Between each of the metallic contact-pins there is a resistance varying from a small fraction of an ohm for the first coil to some hundreds of ohms for the last. The effect is, then, that resistance at a *perfectly definite* rate is added to the circuit before breaking it.

These resistances are adjusted partly by calculation and partly by trial; so that with the particular rate of rotation which the coiled spring S gives to the barrel of the key, the circuit of an A Gramme machine sending a fairly strong current can be broken by the use of this key without any visible spark.

The spring S, combined with the locking-arrangement, makes it impossible for the key to be left partly on or off. If not put so that the current flows without resistance, then the spring S breaks the circuit altogether.

Besides the advantage this key affords of being able to break an entire electric-light circuit without spark, it is possible that another advantage may be produced, viz. that the life of incandescent lamps may be increased by the current passing through them being always gradually diminished to nothing, instead of being stopped as is at present the common practice; but whether this second advantage exists, which, however, seems extremely probable, time of course alone can prove.

At any rate the existence of a Non-Sparking Key now makes it possible for Insurance Companies to turn their attention to the question of prohibiting sparking altogether, which hitherto it has been almost useless contemplating.

VI. *On a Practical Point in connexion with the Comparison of Resistances.* By W. N. Shaw, M.A., *Emmanuel College, Cambridge**.

[Plate IV.]

The most accurate method at present in use for the comparison of two resistances differing only by a small fraction of an ohm, is that suggested by Prof. Carey Foster (Journal Soc. Tel. Engineers, 1872). It is a method peculiarly suitable for the comparison of standards of resistance and the determination of temperature-coefficients of coils. It gives the difference of two resistances compared, free from errors, which might arise in the ordinary sliding-wire Wheatstone-bridge arrangement, from uncertainties as to the contacts of the ends of the slide-wire, or from the resistance of the connecting-pieces between the ends of the slide-wire and the electrodes of the coils compared. In the process of making a determination by this method, the two coils X and Y have to be made to interchange their positions, with reference to the other resistances of the bridge; and it therefore becomes important for the application of the method that the observer should be able to interchange the connexions of the two coils with rapidity and facility.

This is very efficiently and conveniently provided for in the modified form of the slide-wire bridge devised by Dr. Fleming, and described in the third volume of the Proceedings of the Physical Society (p. 174)†. With the ordinary form of bridge the interchange is more difficult, for the coils to be compared have generally to be kept in water, and the two pairs of binding-screws of the bridge are a considerable distance apart, and thus the shifting of the coils to change their connexions is a somewhat inconvenient matter.

At the meeting of the Physical Society on Feb. 23, Prof. S. P. Thompson made some suggestions as to the manner in which the ordinary slide-wire bridge might be adapted so that Carey Foster's method could be conveniently applied.

We have, during the past three years, at the Cavendish Laboratory frequently had occasion to make use of the ordinary bridge for comparing coils and determining temperature-

* Read March 8, 1884.

† Phil. Mag. [5] vol. ix. p. 109.

coefficients ; and have accordingly found it necessary to devise some simple arrangement by which the easy and rapid interchange of the resistances under comparison could be provided for.

I. A simple plan of securing the facility of interchange of contacts is to arrange four mercury-cups in a row, as 1, 2, 3, 4 in fig. 1, and connect them, as shown in the figure, with the binding-screws A B and C D of the bridge by stout copper rods. The mercury-cups should have thick copper bottoms, or, if the arrangement is to be permanent, may be formed conveniently of short bars of copper surmounted by pieces of india-rubber tube, so that connecting-rods can then be permanently fixed to the copper bars. The electrodes of the coils X and Y are inserted at 1 and 3, and 2 and 4 respectively, or *vice versâ*, and their connexions can accordingly be interchanged with very little motion. The electrodes can be kept in contact with the bottoms of the mercury-cups by elastic bands.

This plan of adapting the bridge differs but little from that described by Prof. Thompson, but is, perhaps, somewhat simpler.

II. The second method which we have been in the habit of using requires a more elaborate apparatus, but has the advantage that the contacts of the electrodes of the coils themselves are not altered during an experiment, the interchange being effected by means of a key. The coils under examination can, indeed, be soldered to projecting parts of the key if necessary, and the requisite contacts are secured by springs pressing down metal tongues into mercury-cups. The advantage of this arrangement is very considerable when experiments are being made by observers who are unfamiliar with the eccentricities of contacts.

The arrangement of the key is shown in plan in fig. 2, and in section in fig. 3. Eight mercury-cups 1, 2, 3, 4, 5, 6, 7, 8 are formed by boring holes in an ebonite plate, about half an inch thick, and screwing copper plates on the back (shown by dotted lines in the figure). These copper plates, which are carefully insulated from each other, connect the eight mercury-cups in pairs, and are brought out in tongues A, B, C, D, to which rods, forming connexions with the binding-screws of the bridge, can be soldered, or temporary contacts can be

made by means of the mercury-cups formed by drilling the ebonite, as shown in the figure.

To another plate of ebonite, fixed opposite to these cups on the same board, four pairs of thick tongues of copper—I, II, III, IV, V, VI, VII, VIII—are attached, the ends of the tongues forming springs about 6 inches long. At the end of each spring a copper plunger is arranged to dip down to the bottom of the corresponding mercury-cup, into which it is pressed by a spring, S, as shown in fig. 3. The springs work against the crosspiece of wood M, M, and are insulated from the tongues by india-rubber.

A quadruple crank, Q, is arranged to be rotated between the supports of the crosspiece M, M by means of the handle H. The crank is put under the tongues at the points where they are pressed by the springs. By rotating this crank four alternate plungers (I, III, V, VII) are lifted out of their mercury-cups, the other four (II, IV, VI, VIII) being pressed in by the springs; or, on turning the crank through 180°, the four tongues (II, IV, VI, VIII) are lifted out and the other four pressed into contact by their springs.

The electrodes of the resistances to be compared are introduced into the mercury-cups α, β and γ, δ respectively, or they could be soldered to continuations of the tongues with which these cups are connected.

Thus rotating the crank determines one of the following series of connexions:—

1st position.	Contacts......	1 I.	3 III.	5 V.	7 VII.
	Connexions	A α.	B β.	C γ.	D δ.
2nd position.	Contacts......	2 II.	4 IV.	6 VI.	8 VIII.
	Connexions	A δ.	B γ.	C β.	D α.

So that the coils X and Y are alternately connected with A B and C D, and *vice versâ*, by a half-rotation of the crank.

It is necessary that any method of interchanging the coils should not alter the resistance of the connexions. It will be seen that the arrangement described is quite symmetrical, and therefore satisfactory in this respect, except for the piece of copper connecting I, II with δ; and the resistance of this, if the copper be fairly thick, is too small to be appreciable.

Cavendish Laboratory, Cambridge,
March 7, 1884.

VII. *Note on* Hall's *Phenomenon.*
By Herbert Tomlinson, *B.A.**

In a paper entitled "The Influence of Stress and Strain on the Action of Physical Forces" †, I drew attention to the fact that there is a marked resemblance between the table of "rotational coefficients" drawn up Prof. Hall and that laid down by myself from the results of experiments on the effects of mechanical stress on the specific electrical resistance of metals. A comparison of the two tables given below shows that, with the exception of platinum, the metals stand in nearly the same order in both, and that iron and nickel are very conspicuous, the one at the top and the other at the bottom of the lists. The last-named metal is especially worthy of notice, because in it longitudinal traction, when not carried beyond a certain limit, diminishes the electrical resistance in spite of the increase of length and diminution of section which ensues.

Name of Metal.	Rotational coefficient ‡.	Temporary alteration of specific resistance per unit produced by temporary increase of length per unit. + signifies increase of resistance on application of stress.
Iron	+ 78·0	+2·618
Zinc	+ 15·0	+2·113
Tin	− 0·2	+1·630
Lead	0	+1·613
Platinum	− 2·4	+2·239
Silver	− 8·6	+1·617
Copper	− 10·0	+1·005
Aluminium	− 50·0	−0·420
Nickel	−120·0	−8·860

It might then be suspected that the results obtained by Hall are capable of receiving an explanation, in the fact that the

* Read March 22, 1884.

† Phil. Trans. Part I. 1883, p. 168.

‡ A + sign attached to a metal signifies that the effect is in a direction the same as that which the conductor itself bearing the current would follow if free to move across the lines of magnetic force under the action of the ordinary "ponderomotive" force

electrical resistance of the strips of metal used by him would be altered by the mechanical strain consequent on their endeavour to move across the lines of magnetic force. Mr. Shelford Bidwell has, however, in his recent interesting communications to the Royal Society and the Physical Society, pointed out that "Hall's phenomenon" cannot be thus accounted for. Nevertheless, as it seems not improbable that there is some indirect connexion between the alteration of electrical resistance produced by mechanical stress and the "Hall effect," I have little doubt but that experiments on the "rotational coefficient" of nickel, in which the temperature is varied up to 100° C., or in which the strips are subjected to mechanical traction of varied amount, would assist in deciding the question as to whether purely mechanical stresses are to be regarded as producing the phenomenon. For it has been shown* that with this metal the maximum diminution of resistance following from longitudinal traction is at 100° C. *less than one half* of that at 15°; and, further†, that whilst traction not exceeding a certain limit produces diminution of resistance, increase of stress beyond this limit is followed by increase of resistance. Hall has remarked, in the 'Philosophical Magazine' for September 1881, that the transverse current obtained with a nickel strip is much *increased*, other conditions remaining unchanged, by rise of temperature. But his experiments in this direction are, I think, hardly conclusive; and, indeed, an examination of the numbers given in the column in which the values of the "rotational coefficient" are recorded shows, in the first *ten out of the eighteen* experiments, evidence that rise of temperature causes a *decrease* of the transverse current. Should Hall's phenomenon prove to result from mechanical strain, I incline to the belief that a rise of temperature from 15° to 100° C. will produce decrease in the transverse current.

The experiments of Mr. Bidwell tend very largely to prove that the "Hall effect" can be explained by the joint action of mechanical strain and certain "Peltier effects." Should this be so, we ought, I think, to be able, by means of two thermo-elements connected with each other and with a delicate gal-

* *Loc. cit.* p. 125. † *Loc. cit.*

vanometer, one element being in a cooled region and the other in a heated region, to put this theory to the test. Or, again, we might cover the strip with some material of bad thermal conductivity and watch for a gradual increase of transverse current with time when the electromagnet is excited, and gradual decrease when the electromagnetic stress is removed. Indeed in the actual arrangement adopted by Hall and Bidwell, it would seem that this gradual increase or decrease should occur; for though the strips themselves would rapidly cool if unmounted and exposed to the air, the nonconducting material of the mounting would take some little time both to gain and lose the heat imparted to it by the strip.